The Review of Austrian Economics

Volume 3

Murray N. Rothbard
Walter Block

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Volume 3

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In Memory

A Tribute to W.H. Hutt

Morgan O. Reynolds

One of the great economists of our age, W.H. Hutt, died in June, 1988, just two months shy of his eighty-ninth birthday. As he once told me, "I was born in the century of Napoleon." The comment typified Bill Hutt's amiable pride and gentle character.

Hutt's admirers have long lamented their hero's lack of fame and influence—no awards, knighthood, or Nobel prize. Yet it was partly owing to Hutt's fateful decision to depart his native England for South Africa in 1928, a career venue almost guaranteed to ensure academic obscurity. In recent years, however, a distinct Hutt revival has been under way. And the timeless quality of Hutt's contributions to our body of knowledge promises a growing Hutt impact.

Hutt's academic career began with a 1926 *Economica* article exposing the myth that mass production methods were antiworker in "The Factory System of the Early Nineteenth Century," since reprinted in Hayek's *Capitalism and the Historians*. Hutt was amused and pleased that he continued to receive about forty dollars a year in royalties from a sixty-year-old paper.

The article was only the beginning. His minor classic, *The Theory of Collective Bargaining* (1930), effectively debunked the errors of countless labor writers and economists by demonstrating the antilabor consequences of union coercion. *Economists and the Public* (1936) unfortunately was swept away by the Keynesian tide; *The Theory of Ideal Resources* (1939), perhaps Hutt's most original contribution to economics, was a general theory of unemployed resources clearly superior to Keynes' theory. *The Plan for Reconstruction* (1943) was a valiant if utopian scheme to dismantle market impediments in the postwar world. His important 1954 article, "The Yield from Money Held," extended Mises' theory by integrating the demand for money assets into the general theory of the consumer. *Keynesianism—Retrospect and Prospect* (1963), later revised as *The Keynesian Episode: A Reassessment* (1979), was a wide-ranging dissection of faulty macroeconomic analysis, and *The Economics of the Colour Bar*

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(1964) identified how market forces dissolve the economic and social inferiority of nonwhites and how political forces tend to perpetuate racial inferiority. *Politically Impossible . . . ?* (1971) challenged economists to speak the unalloyed truth, without regard to public opinion or current political fashion. *The Strike-Threat System* (1973) exhaustively showed that union aggression in the long run cannot deliver on its claim to redistribute income from the owners of capital to owners of labor services, and *A Rehabilitation of Say's Law* (1975) straightened out Keynes' distortions of Say's law and focused on the real villain in deficient employment and output: "defects in the pricing system."

Hutt's key ideas—that fiscal and monetary policy cannot offset pricing problems, except temporarily; that restraints on market competition impoverish the poor and disadvantaged rather than helping them; that free-market pricing is the only device available to coordinate and maximize employment and output; that politically expedient concessions to sectional interest groups harm the social interest; that every increase in employment and output via a price cut adds to the source of demands for noncompeting outputs and employment; and that free markets maximize employment and output and diminish inequality generally—no longer wander as intellectual outcasts, beyond consideration in civilized quarters.

Hutt was a familiar figure at international conferences like the Mont Pelerin Society, an organization in which he was a founding member along with luminaries such as Ludwig von Mises, Friedrich von Hayek, and Milton Friedman. Professor Mises once said, "Professor Hutt's rank among the outstanding economists of our age is not contested by any competent critic."

Hutt was a literal citizen of the world, a man who had traveled to every continent and whose work was known on every continent. As a scholar, perhaps courage was his most outstanding characteristic. He was unswervingly devoted to the discovery of the truth, regardless of public opinion, professional fashion, or temporary political sentiment. He was completely detached from sectional interests and a master of rational thought, as the best economists have always been. He always perceived that competition was the shield and support of the general interest, as well as an egalitarian force. He took extraordinary care with definitions, exemplifying the highest standards and rigor of thought. But he always was modest, forever denying originality. He insisted that he was merely clarifying what he thought was "pure orthodoxy" of the pre-Keynesian kind. He truly had the manners and style of another era.

W.H. Hutt was a classical political economist who brilliantly applied and extended economic theory in a variety of contexts. Hutt always put the consumer and entrepreneur at the center of economic action, thereby avoiding the fundamental error embedded in the British classical tradition. Hutt, for example, can be credited with coining the phrase, "consumer sovereignty." True, Hutt's pen was not always facile, he had a penchant for peculiar terminology, and his contributions were not error-free. Yet each work he left behind displays his

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fearless courage, unswerving dedication to the pursuit of truth, and the power of simple economic theory in the hands of a master.

Walter Bagehot wrote that "no real Englishman in his secret soul was ever sorry for the death of an economist." Let us hope that Bagehot's proposition is wrong in Hutt's case. Bill Hutt, a man of gentle courage and scholarly integrity, is gone but his ideas will live on.

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Articles

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The Austrian Theory of the Business Cycle in the Light of Modern Macroeconomics

Roger W. Garrison

The Austrian theory of the business cycle has many critics. Some believe that this part of the Austrian contribution is so misdirected as to constitute an "embarrassing excrescence" (Yeager [1986, p. 378]); others simply doubt that there can be a single theory that provides a general account of cyclical activity (Leijonhufvud [1984, 1986]; see also Sirkin [1972] and Lachmann [1978]); and still others deny the existence of some of the most salient features of business cycles.[1] Defending—or even discussing—the Austrian theory of the business cycle, then, requires some careful groundwork.

There are a number of expositions of the Austrian theory in the literature, which for the most part are complementary (e.g., Hayek [1967, 1975b], Mises [1966, pp. 538–86], Mises et al. [1983], O'Driscoll [1977], Robbins [1934], and Rothbard [1975]), but because business cycles remain a live issue inside as well as outside the Austrian school, there is no—and can be no—canonical version. Gordon Tullock, who took an exposition by Murray Rothbard to be canonical, has identified perceived shortcomings of the Austrian theory in an article entitled "Why the Austrians Are Wrong About Depressions" (Tullock, [1987]).[2] The present article was initially motivated by Tullock's basic objections as well as by his "nit picks," as he calls them. But even his title is evidence of a misunderstanding. The Austrian theory is not primarily about depressions; it is about artificial booms and about the market process that brings them to an end. The theory sheds light on the kind of readjustments needed on the eve of the bust, but the issue of the depth and length of the ensuing depression as measured by the massive unemployment of labor is dealt with by the Austrians in ways that are similar to several other schools of thought.

Though inspired by Tullock's critique, the present article has an organization of its own—one that is responsive to other modern critics as well. Section I

This article incorporates many of the suggestions made by Don Bellante (University of South Florida); Don Boudreaux (George Mason University); Roger Koppl, Michael Montgomery, and Parth Shah (Auburn University); Sven Thommesen (UCLA); and an anonymous referee. Their assistance is gratefully acknowledged.

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considers the very existence of business cycles in order to lay the groundwork for evaluating the Austrian theorizing about them. Section II identifies essential differences between allocative distortions caused by legislation and allocative distortions caused by monetary expansion, linking the latter with cyclical characteristics of resource movements. Section III establishes the significance of capital theory in theorizing about the business cycle. Section IV after that provides some justification for the Austrian approach by considering how rival schools theorize in lieu of a theory of capital. Section V offers a summary evaluation.

I. The Existence of Business Cycles

Business Cycles as Econo-Rhythms

There is a sense in which it can be claimed that business cycles do not exist. If by cycles we mean continuous rhythmic movements in macroeconomic magnitudes, then there are no business cycles. The so-called long wave supposedly identified by Nikolai Kondratieff on the basis of two and a half cycles is the product of creative empiricism and has no basis whatever in theory (See Rothbard [1978], who evaluates Shuman and Rosenau [1972].) Short waves traced out by chartists, or technicians, are equally baseless. Their triple peaks, triple troughs, heads and shoulders, and the like are no more real than faces in the clouds.

Casual inspection of statistical data for economic aggregates such as total output, employment of labor, or net investment suggests a lack of cyclical regularity. Apart from obvious seasonal variations in some sectors of the economy (variations that require no special explanation), it is futile to attempt to identify a frequency and amplitude of some supposed sinusoidal movement. It can be—and has been—argued that the economy is much too complex for any one particular wave to be readily observed. Economic activity is characterized, according to Schumpeter (1934), for instance, by a number of cyclical movements of different frequencies and amplitudes. Kondratieff, Juglar, and Kitchin called attention to the existence of cycles with frequencies of fifty years, nine years, and five years, respectively. Schumpeter suggested that actual patterns of economic activity reflect the combined effects of all such cycles.

But theories about a composite cycle are no more sound than the theory of each of the component waves. Further, any supposed confirmation on empirical grounds of the Schumpeterian view is inherently misleading. As a purely mathematical matter, *any* single-valued function, such as the time pattern of some economic magnitude, can be represented by a Fourier series, which combines an infinite number of sinusoids of different frequencies and amplitudes. The coefficients of a specific Fourier series may describe some particular set

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of macroeconomic data, but if the economics of business cycles is to be more than descriptive, cyclical movements must qualify as such on theoretical grounds.

Business Cycles as Monetary Disequilibria

While there are no built-in econo-rhythms in the market process, there are, from time to time, economywide disturbances of one sort or another. Attempts to identify rhythmic components in economic activity, according to Sirkin (1972) and others, are just misguided attempts to understand these macromaladies. Axel Leijonhufvud (1984) has made headway toward our understanding of macromaladies and of competing theories about them by creating a useful taxonomy. Basic categories are defined in terms of (1) the nature of the disturbance and (2) the nature of the failure of the economy to adjust to the disturbance. The two "natures" are then categorized as *n* (for nominal) or *r* (for real).

This approach gives rise to a two-way taxonomy that can be symbolized as *n/n, n/r, r/n,* and *r/r.* To illustrate, suppose that there is a general, but unanticipated, shift of preferences on the part of wealth holders to a higher level of real cash balances. This is a *real* disturbance. Suppose further that there is some difficulty in the pricing mechanism for both inputs and outputs, which impedes the necessary decrease in the general price level. This is a *nominal* failure. Until the pricing difficulty is overcome, there will be excess supplies of commodities and factors of production on an economywide basis. This macromalady belongs to the *r/n* category.

Leijonhufvud recognizes that his categories represent the pure cases and that it is possible to have disturbances and adjustment failures where both are partly nominal and partly real. He demonstrates, though, that the taxonomy is useful in sorting out the sequential rounds of debate between Keynesians and Monetarists. More generally speaking, historians of economic thought armed with Leijonhufvud's categories can readily detect when opposing theorists are simply talking past one another (theorizing about different sorts of disturbances) and when they have actual disagreements (about how the market reacts to a particular sort of disturbance). Leijonhufvud also shows that the historical relevance of macromaladies of a particular category depends critically upon the nature of the existing policy regime.

Leland Yeager (1986) draws attention to a particular sort of disturbance followed by adjustment failure that, in his judgment, is especially relevant for understanding depressions and hence for devising institutional reform aimed at avoiding them. A decrease in the money supply in the face of an unchanged money demand causes the prices of all commodities and factors of production to be above their market-clearing levels. While the market can eventually bring prices into line with the smaller money supply, it cannot achieve a new monetary equilibrium quickly or painlessly. The theory of "monetary disequilibrium"—a term with which Yeager ties his own ideas to those of Clark Warburton (1966)

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—focuses on the difficulties of achieving economywide price adjustments made necessary by a monetary contraction. Clearly, this focus puts monetary disequilibrium theory in the *n/n* category of Leijonhufvud's taxonomy.

The market's inability to bring about rapid adjustments in prices on an economywide basis guarantees that quantity adjustments will occur instead. That is, the failure—or sluggishness—of nominal equilibration brings on real disequilibration. And as is recognized in almost all macroeconomic theories, a decrease in real output can, through an income-constrained process, induce further decreases. Keynesians envision a "spiraling downward" of income and expenditures. In Yeager's terminology, "the rot can snowball" (1986, p. 371). Austrians refer to this same phenomenon as a "secondary depression" (Hayek [1975a, p. 44]), a term which reminds us that the primary maladjustment is something else.

In monetary disequilibrium theory, the problems caused by price-level sluggishness are further compounded by the fact that not all prices are equally sluggish. A gradual and uneven adjustment in the price level creates a period during which relative prices are pushed away from their equilibrium levels. (The *n/n* malady is contagious and can easily spread to the *n/r* category.) By the time the market reestablishes an equilibrium in terms of both relative and absolute prices, the economy can suffer substantial losses in terms of both misallocated and unallocated resources.

For those who take their cue from Warburton, monetary disequilibrium theory is believed to have broad historical applicability. Any economic downturn involving a monetary contraction is to be understood in terms of the fundamental difficulties of price-level adjustment. Such difficulties should dominate, in this view, any reasonable account of the Great Depression. The theory sheds no light on the problems inherent in a credit-driven boom such as occurred during the 1920s, and it does not explain—nor does it purport to explain—why the money supply began to fall at the end of the 1920s or why there was a prolonged monetary contraction spanning the years 1929–33. It does, however, identify one of the reasons for the economy's poor performance during and immediately after the contraction.

Business Cycles as Self-Reversing Market Processes

In the first view spelled out, business cycles are an inherent part of the market process; in the second, they are disruptions of the market process. That is, both the lower turning point (the upturn) and the upper turning point (the downturn) are *endogenous* for those who conceive of business cycles as econo-rhythms, and *exogenous* for those who think in terms of monetary disequilibrium. Contrasting econo-rhythms and monetary disequilibria in this way suggests another, more conventional taxonomy, in which business-cycle theories are categorized on the basis of the exogeneity (X) or endogeneity (N) of the lower and upper turning points (Hansen [1951, p. 411ff.]). The four categories

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can be symbolized as *X/X, X/N, N/X,* and *N/N,* where *X/X* is monetary disequilibrium theory and *N/N* is econo-rhythm theory. It is difficult to identify any simple relationship between this taxonomy and the one devised by Leijonhufvud. However insightful his treatment of market adjustments to monetary disturbances, Leijonhufvud never explains how—or suggests that—a boom engenders a bust or vice versa.[3]

The Austrian theory of the business cycle falls squarely into the *X/N* category. The exogeneity of the upturn is a clear recognition that the economywide disturbance is inflicted on the market process and is not an unavoidable feature of market economies. The endogeneity of the downturn gives a cyclical quality to the movements in prices and quantities and to certain macroeconomic magnitudes. The Austrian business cycle, then, is less of a cycle than the supposed econo-rhythms, but more of a cycle than sluggish-price monetary disequilibria.

In the broadest terms, the Austrian theory is a recognition that an extramarket force (the central bank) can initiate an artificial, or unstainable, economic boom. The money-induced boom contains the seeds of its own undoing: the upturn must, by the logic of the market forces set in motion, be followed by a downturn. Note that the words *induced* and *unsustainable* are consistent with the *X* and *N*, respectively, that define the *X/N* category of business-cycle theory.

The Austrian theory also qualifies, along with monetary disequilibrium theory, as a monetary theory of the business cycle. "Money matters" in both theories—but for different reasons. Further, if the Leijonhufvud taxonomy is applied to the entire sequence of events from the initial upturn to the subsequent downturn, then the Austrian theory would fall into the *n/r* category. As summarized by Fritz Machlup (1976, p. 23), "*monetary* factors *cause* the cycle but *real* phenomena *constitute* it." For Yeager and Leijonhufvud, monetary mismanagement precipitates a bust; for Mises and Hayek, monetary expansion engenders a boom, which eventually leads to a bust.

The Austrian theory is to be fundamentally distinguished from monetary disequilibrium theory by its emphasizing that "relative prices matter." The more prevalent claim that "money matters" derives from considerations of money-induced changes in the price level and sometimes of changing relative prices as the market process makes piecemeal adjustments toward monetary equilibrium. But for the Austrians, relative-price changes form the core of the theory. Money-induced changes in relative prices cause corresponding changes in the pattern of resource allocation. The self-reversing character of the market process set in motion by the injection of newly created money manifests itself most significantly in that aspect of the process that allocates resources over time—in the intertemporal structure of capital as governed by the interest rate. Alternatively stated, the observed cyclical quality of the market process consists in a *temporary* disruption of *intertemporal* market mechanisms.

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II. Legislated Distortions and Monetary Distortions

The government implements all sorts of policies and programs that cause the price of some particular good to be above or below its market level. Interventions in the form of taxes and subsidies, price floors and ceilings, tariffs and quotas are, from a narrowly economic point of view, permanent in their effects on resource allocation. A subsidy to home building, for instance, will result in a larger-than-otherwise investment in housing so long as the subsidy is in effect. It is possible, of course, that some regulatory schemes can create a political dynamic that eventually results in deregulation. Government-enforced cartelization of the airlines, for example, led to an eventual competing away of monopoly profits by the members of the cartel, which eroded the political support for continued regulation. The political forces for deregulation eventually prevailed. But apart from considerations of such political dynamics, legislative interventions by government have a certain permanence about them.

Legislated distortions of the price system play no direct role in cyclical movements of economic magnitudes precisely because of their quality of permanence. By contrast, monetary distortions do play a direct role in business cycles precisely because—and to the extent that—they are inherently impermanent. The market's distribution of income and hence of spending patterns, gives rise to a certain pattern of prices. The pattern can be altered by the spending of newly created money on some particular good or category of goods. But the initial price increases brought about by monetary injections, and more importantly the reallocation of resources associated with those price increases, do not have the permanence of legislated price supports. Subsequent rounds of spending of the newly created money reflect not the policy objectives of the monetary authority, but the preferences of the income earners. Prices not initially affected by the monetary injection are eventually bid up, thus causing a reversal in the movement of resources. Apart from one consideration to be noted later in this section, the allocative effects of a monetary disturbance are necessarily self-reversing.[4]

One of the most common objections to this aspect of the Austrian theory concerns the movements and subsequent countermovements of resources. The initial quantity changes would simply not occur, so the objection goes, if subsequent changes in the opposite direction were anticipated. Although alternative treatments of expectations will be discussed in a subsequent section, it may be helpful at this point to deal in a general way with the problem of expectations in macroeconomic theorizing.

A business cycle anticipated, in the view of some macrotheorists, is a business cycle avoided. Employing the assumption of so-called rational expectations along with other essential assumptions, such as instantaneous market clearing and costless information, the New Classicists are able to transform the impermanence of money-induced distortions as seen by the Austrians into

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the nonexistence of such distortions. The self-reversing process becomes a self-preventing process. The Austrian focus on the injection effects of monetary expansion (rather than the price-level effects) and on the market process set into motion by the monetary injections warns against adopting the New Classicist view. Several considerations are relevant.

First, conceiving of monetary expansion as a process involving sequential rounds of spending suggests that expectations, even if correct or rational, may not preclude the cyclical effects of monetary expansion. Whatever their particular expectations, individuals who receive the newly created money only in later rounds have less spending power than those involved in early rounds of spending. Although for any individual, the ability to spend is not strictly limited to the amount of money currently possessed, there are ultimate limits on the individual's ability to transform expectations into actions. Put bluntly, you can't spend expectations. While bank credit and trade credit can provide substantial leeway, spending is not *perfectly* elastic with respect to unborrowed money balances (Hayek [1978, p. 175]). Thus, individuals who correctly and rationally expect a large injection of newly created money are not necessarily in a position to act in full accordance with their views—however rational those views ultimately turn out to be.

Second and more important, individuals who are in possession of increased money balances and who have correct, or rational, expectations still may not spend in a pattern consistent with the New Classicist view. A spending pattern that is internally inconsistent on an economywide basis does not necessarily imply inconsistency for the individual. That is, macroeconomic irrationality does not imply individual irrationality. An individual can rationally choose to initiate or perpetuate a chain letter—sending one dollar to the person on the top of the list, adding his name to the bottom, and mailing the letter to a dozen other individuals—even though he knows that the pyramiding is ultimately unsustainable. Similarly, it is possible for the individual to profit by his participation in a market process that is—and is known by that individual to be—an ill-fated process. So long as it is possible to buy in and sell out before the process reverses itself, rational expectations may exacerbate rather than ameliorate the misallocation of resources induced by monetary expansion.

Third, apart from the relative-price changes that are reversed in a subsequent part of the process, there remains an effect that persists so long as the monetary authority continues to inject money in some particular way. (If this were the dominant effect, monetary distortions would be similar to legislated distortions and would lose their cyclical quality.) To illustrate with an extreme example, suppose that an aggressive and sustained monetary expansion is accomplished solely through the purchase of home mortgages. Can anyone doubt that the allocation of credit among borrowers and of resources among construction projects would be permanently affected? Hayek clearly recognized the permanence of this particular effect of monetary injection by using

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the term *fluid equilibrium* (1978, p. 173). So long as the monetary authority maintains its spending level in real terms, which in view of the resulting inflation requires an exponentially increasing level of spending measured in nominal terms, the distortion remains.[5]

Considerations of a possible fluid equilibrium, of disparities between rationality as applied to the individual and as imputed to the economy, and of limits to the transformation of expectations into actions all warn against the New Classicist view. It is simply not true that full knowledge of a monetary expansion is tantamount to no expansion at all.[6]

Austrians are sometimes criticized for assuming static expectations—the clear implication being that the assumption of rational, or even adaptive, expectations is preferable. This criticism would have some validity if a change in the assumption about expectations—from static to adaptive to rational—were to nullify the theory or cause it to have categorically different implications. But such is not the case. The assumption of static expectations, when employed, serves as a heuristic device. The market forces that characterize a money-induced boom and the subsequent bust can be spelled out first in their simplest form. Amendments can then be made to account for complications that arise from other-than-static expectations. The assumption of adaptive expectations requires that the arguments be restated replacing monetary injections with rates of monetary injection and then with accelerations, surges, and so on as market participants continue to adapt. The assumption of rational expectations, in its most defensible form, requires that the basic truth in Lincoln's law (You can't fool all the people all of the time) be recognized—as it was recognized by Mises (1953, p. 319) long before the birth of New Classicism. The assumption of rational expectations in its least defensible form (You can't fool any of the people any of the time) is to be dismissed out of hand.

III. The Significance of Capital in Business-Cycle Theory

The self-reversing market process set into motion by monetary expansion, previously described in general terms, begins to take on a more specific character when spelled out in the context of some particular market. If the analysis is to retain its macroeconomic character—that is, if the self-reversal is to have economywide ramifications—then the focus must be on some broadly defined market such as the market for labor, the market for capital, or the even more broadly conceived market for productive factors. If, however, the market that serves as the focus of analysis is defined too broadly, such as the all-inclusive market for goods, then there can be no money-induced process of any significance and hence no reversal. In a theory where holding money and buying

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undifferentiable goods are the only two alternatives, business cycles would be trivially portrayed—using Irving Fisher's imagery—as the "dance of the dollar." This is the fate of business-cycle theory, for instance, in the four-sector model devised by Patinkin [1965].

Capital in the Austrian Theory

That the Austrians singled out the market for capital goods as their focus for business-cycle analysis is to be accounted for by several considerations. First, it was largely the observed and widely acknowledged movements in capital-goods markets that initially motivated a theoretical explanation. Significantly, the various competing schools of thought—including the Austrians—used the terms *business cycle* and *industrial fluctuation* synonymously. The idleness of producers' goods used in heavy industry was perceived to be one of the most obvious and dramatic characteristics of economic downturns.[7]

Second, as a historical and institutional matter, monetary injections take the form of credit expansion. That is, newly created money is put into circulation through credit markets. In this respect too, the Austrian theory has a stronger empirical content than rival theories. The conventional assumption that newly created money is added directly to the cash balances of market participants serves to abstract from the market process highlighted by the Austrians. Increased real cash balances of all individuals mean an upward pressure on all prices. But in the Austrian formulation, the spending of newly created money does not impinge on all prices at once or in some random fashion; it impinges in the first instance on the interest rate, the price that clears the market for credit and governs the allocation of capital.

Monetary expansion temporarily alters the terms of trade between goods now and goods later. This money-induced alteration has its most direct effect within the market for capital goods. The capital goods themselves constitute current commitments, some more binding than others, to particular production processes. In general terms, a fall in the rate of interest stimulates the creation and use of capital goods that aid in the production of consumer goods in the relatively remote future at the expense of those that aid in the production of consumer goods in the relatively near future. But the movement of resources away from the production of lower-order capital goods and toward the production of higher-order capital goods is followed by a countermovement (Mises [1953, p. 363]). That is, the money-induced restructuring within the market for capital goods is eventually revealed to be inconsistent with intertemporal consumer demand and resource availabilities; the process is self-reversing.

Third, it is the temporal dimension of capital that gives scope and significance to the money-induced self-reversing process. The essential function of capital, pointed out early on by Jevons, is "to put an interval between the beginning and the end of enterprise" (1970, p. 226). This, in summary terms, is the

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interval of time during which a misallocation of capital goods can occur and after which a reallocation must take place. Alternatively stated, it is the interval itself that is thrown out of equilibrium by credit conditions that are at odds with resource availabilities.

The economy's production process that spans the Jevonian interval consists of a number of separate stages of production. This vertical segregation, or temporal sequencing, comes into play in a way that is not always recognized. If all production processes were characterized by complete vertical integration—such that the commitment to initiate a process that will eventually result in the production of a consumer good is, in effect, a commitment to complete it—there would be little or no scope for a self-reversing process. Many of the arguments against the Austrian theory based on considerations of expectations would have greater plausibility. Entrepreneurs who anticipate the ultimate consequences of easy money—on the basis of either theoretical understanding or historical experience—would not be eager to participate in a money-induced boom. Those who continue to produce despite the monetary disturbance would compete with one another at the outset for lines of credit that would see their production process through to completion.[8]

Identifying the circumstances under which expectations would be potentially nullifying helps to explain why expectations are not actually nullifying in modern industrial economies. Neither chain letters nor money-induced production processes would be initiated if their initiators were bound to participate in every subsequent stage of the respective processes. The absence of complete vertical integration, however, can create significant opportunities for entrepreneurs to profit privately from one or more stages of a production process that, taken as a whole, will result in a social loss. And, as in the case of chain letters, those who make profits in the early stages may or may not hold expectations that reflect an understanding of the nature of the process; expectations, rational or otherwise, are in this context a subsidiary issue.

Still again, the Austrian theory has empirical content that is absent from rival theories. Primitive societies, whose members live a hand-to-mouth existence, do not experience business cycles as described by the Austrian theory; they have no capital structure that can become intertemporally discoordinated. Labor-intensive agricultural economies, whose intertemporal structure of production is determined more by the seasons than by credit conditions, are largely immune to the cyclical disturbances identified by the Austrians. Susceptibility to money-induced self-reversing market processes increases with the interval between the beginning and the end of enterprise and with the extent to which production processes are divided into temporally sequenced stages of production. These propositions conform with the broadly empirical observation that the boom-bust pattern to which the Austrian theory applies is characteristic of capital-intensive, market-oriented economies with a centrally directed monetary system.

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That the Austrians were and continue to be the *only* school to focus on the market for capital when theorizing about business cycles is also understandable. They were the only school that had a well-developed capital theory. Menger (1950) identified the different orders of goods in accordance with their temporal sequence in the production process and drew attention to the intertemporal complementary that influenced the goods' value. Böhm-Bawerk (1959) dealt with the time element in terms of "roundaboutness" and demonstrated the inverse relationship between the rate of interest and the degree of roundaboutness that characterizes the economy's production process. Mises (1953) integrated monetary theory and value theory by developing Wicksell's distinction between the bank rate of interest and the so-called natural rate in the context of Böhm-Bawerk's capital theory. The Austrian theory of the business cycle was a natural outgrowth of these developments.[9]

Capital in Rival Theories

Rival theories either had no capital theory at all or had a capital theory that did not integrate well with monetary theory. In the 1930s, Keynes (1964, p. 176) rejected Böhm-Bawerk's theory out of hand—without providing a serious critique of it or even demonstrating that he understood just what that theory entailed. But with the Austrian theory jettisoned, Keynes did not attempt to offer an alternative. As was made clear in reference to his earlier theorizing, the attempt, instead, was to press on with the macroeconomic issues in the absence of capital theory (Keynes [1931, p. 394f.]).

After several decades of macroeconomics without capital, the Monetarists were able to expose many of the fallacies and shortcomings of Keynesian theory. But they were unable to identify those shortcomings that derive from the neglect of capital theory. Monetarism embraced a theory of capital and interest put forth by Frank Knight (e.g., 1934), who had engaged in a tedious and protracted debate with Hayek and other members of the Austrian school. Knight could make no sense of Jevons' interval or of Böhm-Bawerk's roundaboutness. Production and consumption, in the Knightian conception, are not temporally distinct activities. The only relevant distinction, according to Knight, is that between the economic flows of income or utility and the corresponding stocks into which such flows can be capitalized. But to conceive, as Knight did, of capital and interest as nothing but permanent stocks with automatic flows is to abstract from the intertemporal market processes that captured the attention of the Austrians and from the monetary disturbances that may interfere with those processes. Knightian capital theory, in the hands of the Monetarists, did not provide an alternative basis for integrating monetary theory and value theory; it provided, instead, a device for keeping the two theories segregated.

In recent years, New Classicism (Lucas [1981], Barro [1981]), with its emphasis on rational expectations, has become the most formidable rival to the

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waning Keynesianism. While the theoretical constructions of the New Classicists differ in fundamental ways from those of the older Monetarists (Hoover [1984]), they share in the neglect of capital theory. These newer constructions highlight the temporal variation in macroeconomic magnitudes, yet the arguments hinge almost exclusively on *wage* rates and the intertemporal allocation of labor. *Interest* rates and the intertemporal allocation of capital are in no fundamental way a part of New Classicism. This incongruity dramatizes the resolve on the part of contemporary macroeconomists not to grapple with theories of capital and interest—even when intertemporal relationships are specifically at issue.

One encouraging development within the New Classical school, however, deserves mention. The assumption of rational expectations, coupled with assumptions of costless information and instantaneous market clearing, implies that a monetary disturbance should not have any systematic real effects beyond the period in which the disturbance occurs. Empirical studies, though, reveal a certain persistence of effects. Some New Classicists (Kydland and Prescott [1982]) attempt to account for this persistence by incorporating "time-to-build" considerations into an otherwise capital-free construction. That is, money-induced decisions to initiate a multiperiod production process affect in systematic ways the decisions to be made in subsequent periods. While time-to-build was added belatedly and only to resolve a disparity between theory and evidence, this development could lead to a reintroduction of capital theory into macroeconomics.

IV. In Lieu of Capital Theory

The Austrians focus on capital markets in their analysis of business cycles while rival schools do not. This much is easily established. But what sort of a macroeconomic construction remains when capital theory is subtracted from business-cycle theory? Answering this question for each of the rival schools helps to identify important differences among them. It also serves further to demonstrate and emphasize the crucial role of capital in the Austrian theory.

Keynesianism

Although Keynes had sympathy neither for Austrian capital theory nor for the Austrian theory of the business cycle, he did not offer alternative theories of his own. Ambitious as his *General Theory* was, it contained only "Sundry Observations on the Nature of Capital" and "Notes on the Trade Cycle," as announced by the titles of chapters 16 and 22. Now, more than half of a century after the book's initial appearance, Keynesian scholars are still debating whether or not the malfunctioning of capital markets is central to Keynesian

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theory. The debate gets resolved as soon as a choice is made between focusing on what Keynes left out of his book and focusing on what he actually put in it. The consequences of each choice can be identified in summary terms.

There are no market mechanisms—at least none identified by Keynes—that can effectively allocate resources intertemporally. The rate of interest is determined by the supply and demand for money; the decision to invest is based, in large part, on the groundless expectations held by the business community, or on animal spirits, to use Keynes' own terminology. Not surprisingly, booms and busts occur with the waxing and waning of business confidence. When confidence is on the wane, the demand for labor falls, resulting in widespread unemployment. Wage rates either (1) will not fall because of unions or because of wage rigidities inherent in the market process, or (2) will fall but without making matters any better and possibly making matters worse because of the accompanying fall in the price level, or (3) should not be allowed to fall because of the considerations mentioned in (2).[10] Macroeconomic problems persist until some set of extramarket forces are designed to counteract the undirected and misdirected forces of the marketplace.

With this interpretation of Keynes, the absence of effective markets for capital goods, which derives by default from the absence of capital theory from Keynes' book, becomes the central focus (Garrison [1985]). If there are no coordinating mechanisms that, even in the best of circumstances, can effectively allocate resources intertemporally, then intertemporal markets will be discoordinated. The conclusion follows trivially. There remains nothing more for capital-oriented Keynesians to do, except for drawing analogies between market economies and kaleidoscopes (Shackle [1974]) or pondering—on the basis of a highly selective exegesis—about what Keynes must have had in the back of his mind (Leijonhufvud [1968]).

As an alternative interpretation, the fact that Keynes' *General Theory* contains no general theory of capital can be taken to imply that his theorizing is based on the assumption of a fixed capital stock and a fixed capital structure (Keynes [1964, pp. 40–45]). This assumption, stated symbolically in textbooks as $K = \overline{K}$, allows the focus of analysis to be shifted to other macroeconomic magnitudes, among which Keynes did posit some definite relationships. Consumption spending rises and falls with—but not as fast as—income: $C = a + bY$ where b is the marginal propensity to consume. This short-run consumption function, in which $a > 0$ and $0 < b < 1$, becomes the keystone of the theory. The remainder of the theory is specified in terms of interest elasticities: the demand for investment funds is interest-inelastic, and the demand for idle money balances is interest-elastic—both perfectly so in the limit.

Keyneisan multipliers, which are based on such propensities and elasticities, relate changes in employment to changes in investment spending. The same relationships hold, in this interpretation, whether the investment is undertaken by the business community or the government. The will to spend rather than

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any more fundamental constraint, such as economic scarcity, is what limits the level of employment and hence national income. The intertemporal pattern of output is traced out as the unpredictable forces in the market for investment goods interact with the largely predictable forces in the market for consumer goods. (Coddington [1982] finds the significance of Keynesian theory in this interaction between the stable and the unstable sectors of a market economy.)

Most modern textbooks on macroeconomics consist of graphs and equations of such relationships gleaned from the *General Theory*'s treatment of money, interest, and employment, given the economy's capital stock. The issue of a changing capital stock is typically relegated to a separate chapter on economic growth, appended almost as an afterthought to the Keynesian chapters. This interpretation of Keynes has given rise to a distinction that stands in the way of reintroducing capital theory into macroeconomics. Macroeconomic theory is implicitly defined as all those relationships that can be identified among macroeconomic magnitudes on the assumption of a fixed capital stock. Theory involving a changing capital stock is, by definition, growth theory.

Writing three decades after the publication of the *General Theory,* John Hicks undertook a telling of the "Hayek story." He recalled the "time when the new theories of Hayek were the principal rival of the new theories of Keynes" (Hicks [1967, p. 203]) and then he justified his own alliance with Keynes on the basis of the modern definitional distinction. According to Hicks, we see in retrospect that Hayek's theories were not relevant to business cycles at all. Monetary disturbances—money masquerading as savings—could not cause the resource movements from consumer-goods industries to producer-goods industries as suggested by Hayek, because those movements involved actual changes in the capital structure. Actual changes in the capital structure can be brought about only by actual changes in the rate of savings. Hayek was theorizing not about business cycles but about economic growth. Not only had Keynesianism prevailed over Austrianism, it had numbed the ability of at least this one modern macroeconomist to think in terms of money-induced movements within the capital structure which constitute an artificial boom and lead—eventually but inevitably—to an economic bust.[11]

Monetarism

Monetarism has come to be closely identified with the quantity theory of money—so closely that it is sometimes defined narrowly in terms of the positive, virtually one-to-one relationship between the money supply and the general price level. "Inflation is always and everywhere a monetary phenomenon." The phraseology is uniquely Monetarist, but the idea itself has long been shared with the Austrians.

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Attempts, even by the Monetarists themselves, to define this school of thought more broadly have been less than satisfying. At one stage of the debate beween the two schools, Friedman (1970) undertook to differentiate Monetarism from Keynesianism by reference to the Keynesian-based income-expenditure analysis. In this context, the key differences derive from differences in elasticities. For the Monetarists, the demand for money is interest-inelastic, the demand for investment funds is interest-elastic. If debate between the two schools resolved itself into such a simple empirical question, it could be settled in short order by consulting the data. If, alternatively, the differences in elasticities are simply a reflection of the short-run orientation of Keynesians and the long-run orientation of Monetarists, then Keynesian-based Monetarism is on weak grounds. The applicability of income-expenditure analysis is restricted by the assumption of a fixed capital stock—an assumption that can hold, if at all, only in the short run.[12]

A more general distinction between the two schools makes reference to underlying *beliefs* about the market system (Leijonhufvud [1981a, p. 297ff.]). Monetarists believe that markets work, that prices and wages are tolerably flexible, that individuals do not suffer from money illusion, and that market expectations will not for long be in conflict with reality. The perversities in the Keyenesian vision stem from disbelief on one or more of these counts.

The contrast of underlying beliefs is especially revealing when applied to a particular market, the market for capital goods. Both Keynesianism (interpreted as income-expenditure analysis) and Monetarism leave capital out of account—but for opposite reasons. For the Keynesians, markets for capital goods are so ill behaved (references in the *General Theory* to casinos and musical chairs are relevant here) that nothing much *can* be said about them; for the Monetarists, markets for capital goods are so well behaved (references to the Knightian vision of synchronous production and consumption are relevant here) that nothing much *need* be said about them. It is worth noting at this point that the Austrians occupy a middle-ground position (as they do on so many other substantive issues). (See Garrison [1982].) Equilibrating forces are at work in the market for capital goods, but they are particularly vulnerable to monetary disturbances. Because of the essential time dimension in the production process, a dimension whose relevance is trivialized by Keynes and denied by Knight, money-induced disequilibrium originating in the early stages of production can persist undetected until the production processes enter their final or near-final stages.[13]

Monetarists and Austrians do share a common ground, however, in that they each focus on a self-reversing process triggered by monetary expansion. But with the structure of capital outside their vision, the Monetarist analysis is focused almost exclusively on the market for labor (as in Friedman [1976]). The analysis of intertemporal distortions spelled out by the Austrians in terms of the various stages of production that make up the Hayekian triangle is supplanted by an analysis of labor-leisure distortions spelled out by the

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Monetarists in terms of the short-run and long-run Phillips curve. The self-reversing nature of the process identified by the Monetarists and hence the analytical kinship to the Austrians, however, is clearly evident. Money-induced movements away from the natural rate of unemployment set into motion a market process in which changes in perceived wage rates and output prices eventually and inevitably reestablish the natural rate. (Material in the next few paragraphs is condensed from Bellante and Garrison [1988].)

The details of the self-reversing process as described by Monetarists differ categorically from those described by Austrians precisely because of the absence in the former of any disturbances within the structure of capital. A time-consuming production process thrown into intertemporal disequilibrium by a monetary injection is no part of Monetarism. Instead, the self-reversing process plays itself out within the market for labor and on the basis of differing perceptions of the effect that inflation has on the real wage rate. More specifically, northwestward movements along a short-run Phillips curve are produced by a labor market in which the worker believes the real wage rate (reckoned in terms of consumer purchasing power) has risen but in which the employer believes the real wage rate (reckoned in terms of the price of the firm's output) has fallen. The inevitable eastward shift of the short-run Phillips curve is brought about when both workers and employers eventually discover that the real wage rate has in fact not changed in either direction. In symmetrical fashion, deflation or even disinflation produces southeastward movements along a short-run Phillips curve followed eventually and inevitably by a westward shift of the curve.

The Monetarists' version of the self-reversing process is less than satisfying on several counts. First, why should injections of newly created money through credit markets, which affect, in a very direct way, interest rates and hence markets for capital goods, have effects of overriding importance on wage rates? Second, how plausible is an account that relies, in one inflationary episode after another, on chronic and systematic misperceptions of the real wage rate? (Note here that the temporally sequenced stages of production that make up the capital structure add a dimension to Austrian theory that has no direct counterpart in Monetarist theory.) And third, why should it take so long in any given inflationary episode for workers and employers to straighten out their misperceptions of the real wage rate?

In addition to lacking plausibility, the Monetarist account grossly understates the consequences of credit expansion. If growth in real output prevents the credit expansion from resulting in an increase in the general level of prices, then there are no misperceptions of wage rates and hence—in the Monetarists' view—there is no money-induced self-reversing process. Further, if the account is confined to unskilled labor, then the misallocations would be short-lived and easily corrected. Any actual welfare loss would manifest itself as a lament on the part of workers that they had consumed either too much or too little leisure as a result of the monetary disturbance. If, alternatively, the misallocation of skilled labor is to be taken into consideration, then the focus is shifted to human capital and

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the Austrian analysis comes into play. The time-consuming development of specific skills and the development of skills complementary to specific long-term production processes are features of a theory that involves an intertemporal structure of both human and nonhuman capital.[14]

New Classicism

A brief stock taking at this point will help to put post-Monetarist developments into perspective. Keynesian theory in all its interpretations makes a first-order distinction between markets for consumer goods, which always perform appropriately in accordance with the fundamentals of supply and demand, and markets for investment goods, which never—or only by accident—perform appropriately. The economy's output as well as the employment of labor in all interpretations of the theory varies in direct proportion to the spending on (private and public) investment goods.

Monetarism, interpreted as Phillips-curve analysis, makes a first-order distinction between capital markets, in which nonhuman resources are allocated efficiently, and labor markets, in which inflation-induced misperceptions of the wage rate can cause temporary but systematic misallocations. The economy's total output in this analysis varies in direct proportion to the employment of labor.

There is no comparable first-order distinction in New Classicism between markets that work right and markets that go wrong. As a first—and sometimes last—approximation, all markets allocate resources efficiently. In the early and hard-drawn expositions of New Classicism, the assumption of rational expectations implied trivially that all markets are governed, in the short run as well as the long run, by the fundamentals of supply and demand. Stabilization policy as might be conceived and implemented by the monetary authority is both ineffective and unnecessary. Thus, rational expectations in its early applications to macroeconomics did not constitute an alternative to Keynesianism and Monetarism; it simply denied the phenomenon (cyclical unemployment) that the Keynesians and the Monetarists were attempting to explain.

To allow the subject matter back into the analysis of it, the New Classicists had to invent a distinction that could drive a wedge between some actual price or quantity and the corresponding equilibrium price or quantity. The distinction that now dominates in models employing the assumption of rational expectations is that between local knowledge and global knowledge (Phelps [1970], Barro [1976]). A system of island economies is conceived in which prices can be affected both by changing supply and demand conditions in each local economy and by money-supply shocks that have consequences on a global scale. Immediate knowledge about nominal price changes coupled with belated knowledge about money-supply changes gives scope for real prices to deviate temporarily from their equilibrium levels. Monetary disturbances, in this construction, can affect the level of output in the local economy to the extent that nominal price changes are mistaken for real price changes.

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The critical role of knowledge—of two kinds of knowledge—allows for an interesting comparison between New Classicism and Austrianism. The New Classicists' objective, often stated with unabashed pride, is to theorize about the economy without recourse to the sort of ad hoc assumptions that characterize other schools of thought. (See e.g., Klamer's conversations with Lucas and Sargent [1983].) Their conclusions do not turn on some supposed rigidity or inflexibility of prices or wages, on some expectational scheme that is at odds with the theory that incorporates it, or on some supposed failure of market participants to take advantage of the knowledge they possess.

The assumption that there is a first-order distinction between knowledge of movements in the money supply and knowledge of other economic magnitudes, however, is just such an ad hoc assumption. What are the relevant constraints and objective functions that determine the length of this lag? Is the lag constant over time? And why should this particular lag in the acquisition of knowledge have significance that overshadows all others—including the one that characterizes the Monetarists' Phillips-curve analysis? While these questions remain unanswered by the New Classicists, their models must incorporate this or some similar knowledge lag in order to avoid absurd or trivial conclusions—that money does not matter or that money does not matter if market participants base their actions on real factors only.

The Austrians, too, employ a knowledge-based distinction, but not one that requires island economies or any other such fictitious construction. Long before the economics of knowledge was an object of attention, Hayek (1948) made a first-order distinction between two kinds of knowledge. Theoretical knowledge about how the economic system works must be treated differently from knowledge of the particular circumstances of time and place. This distinction does not represent an ad hoc assumption, but rather reflects important insights of the earliest political economists. The message conveyed by Adam Smith's "invisible hand" is that the economic system works without the market participants knowing—or caring—just how. Referring either in general to the structure of the economy or in particular to the intertemporal structure of production that serves as a basis for the Austrian theory of the business cycle, the two kinds of knowledge can be identified as knowledge *of* the structure and knowledge *within* the structure. Alternatively, the distinction is between theoretical knowledge and entrepreneurial knowledge. (For a discussion of the relationship between these two kinds of knowledge and the extent of the overlap, see Garrison [1986, p. 444f.].)

Market participants possess enough entrepreneurial knowledge to make the economy work, but they possess little or no theoretical knowledge. The play-off between knowledge *within* and knowledge *of* the structure has the same analytical significance for the Austrian formulation as the play-off between local and global information has for the New Classical formulation. In both, the distinction between two kinds of knowledge allows for the derivations of results

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that conform in some degree to real-world observations. But for the Austrians, the distinction is not just an abstract modeling device; it is a recognition of one of the most fundamental features of real-world market economies.

The full dependence in the rational-expectations models on the time lag between the acquisition of global information relative to local information can be demonstrated by considering the economic structure typically envisioned by the New Classicists (e.g., Barro [1976]). In effect, there is only one commodity being supplied and demanded. The commodity is conceived to be nondurable in the extreme—a service indistinguishable from the corresponding labor that renders it. This construction avoids dealing with any kind of a production *process* or even with a choice between consuming a good now or storing it for later consumption. Also, demanders on any particular island possess the same information as suppliers. This assumption insures that the only information difference that matters is the one between global and local information.

At this point we may legitimately wonder why there would be any trade on such an island? What keeps each individual from consuming his own labor services? There must be something in the nature of the service such that one individual must render it to another. Though several possibilities come to mind, Barro (1976, p. 83) has suggested that we think in terms of "back-scratching services." Trade actually does take place. Still, there is room for more legitimate wondering. What need would such an economy have for a medium of exchange? The usually troubling double coincidence of wants is no coincidence at all: "I'll scratch your back; you scratch mine." At most, money would serve as an accounting device used to keep track of the indebtedness of scratchees to scratchers.

The objective of such primitive models in which individuals live a hand-to-back existence is to facilitate the investigation of the consequences of a monetary injection. And as is conventional in such formulations, it is assumed that the injection takes the form of transfer payments—thus avoiding any interest-rate effects that might occur in a credit expansion. The only possible consequence, then, is that the price of back-scratching services is bid up. The key concern is with how individuals divide their time over the next several periods between rendering the service and consuming leisure as they guess about the cause of the price increase and eventually learn that it is attributable to a monetary shock and not to a change in some real factor (e.g., an increase in itching). Again, there are grounds for wondering. What relevance could an answer to such a question possibly have for understanding the causes of industrial fluctuations in modern, market-oriented, capital-intensive economies?

In the face of such wondering and implied criticism, rational expectations and island economies are defended not as having direct relevance or as highlighting aspects of the market process that are actually crucial in real-world business cycles; they are defended instead simply as modeling techniques for building analogue economies. If some model, by which is meant a "fully articulated

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artificial economy," turns out to generate time patterns of unemployment and output that mimic to a first approximation the actual time series for those magnitudes, then we have attained—by virtue of being able to construct such a model—an understanding of those patterns (Lucas [1981, p. 219]). Though the link between building such economic models and understanding actual economics is often implied and sometimes asserted, the methodological reasoning that establishes the link is, to my knowledge, never revealed.[15]

New Classicism might more palatably be defended by an appeal to instrumentalist methodology. The models themselves provide no understanding, but they can be instrumental in our deciding what correlations and time patterns to look for in the macroeconomic data. By construction, however, the models fail to suggest that business cycles may have something to do with the capital structure as affected by movements in the interest rates.

While New Classicists often claim some affinity to the Austrian school, they reject the Austrian theory of the business cycle strictly on empirical grounds. The magnitude of the alleged cause (cyclical changes in the interest rate) is so small compared to the magnitude of the alleged effect (a crisis in the market for investment goods) that the Austrian theory cannot seriously be entertained (Lucas [1981, p. 237]). It is tempting simply to ignore this criticism of the Austrian theory, pointing out that by similar logic a careless smoker could not possibly cause a forest fire. But because the empirical significance of interest-rate effects is so often in question, a more serious and considered response may be in order.

First, a cyclical pattern in observed interest rates is not essential to the Austrian theory. A money-induced deviation of the loan rate from the natural rate is the exogenous triggering device. Further, central-bank policy that maintains constancy in the easily observed loan rate under conditions in which the not-so-easily observed natural rate has risen can initiate the self-reversing process within the market for capital goods as identified by Austrian theorists.

Second, the effects of an artificially low interest rate are not so much *over*investment as *mal*investment. While the low rate does generally favor investment over consumption, the validity of the Austrian theory does not hinge on the magnitude of this effect in aggregate terms. Low interest also favors particular kinds of investment over others. It favors more durable over less durable capital goods as well as capital goods used in more roundabout rather than less roundabout production processes. These are the effects that are overlooked by simple calculations showing that the demand for investment funds is interest-inelastic.[16]

Third, the crisis manifests itself as intertemporal discoordination that requires a systematic reallocation of capital within the structure of production. Because of the number of relatively long-term production projects undertaken, resource availabilities are not quite sufficient to carry them through. That they are merely not *quite* sufficient is what allows the artificial boom to be sustained

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over a considerable period without its artificiality being apparent. But that they are *not* sufficient is what makes an eventual restructuring inevitable. The realization that the sustainability of production processes on an economywide basis may be threatened by small but prolonged distortion of the interest rate away from its natural level confers plausibility on the Austrian theory of the business cycle.

New Classicism, Monetarism, and Keynesianism each deal in some indirect way with the intertemporal allocation of resources. Even a casual survey reveals, however, that descriptions and discussions of market mechanisms supposedly relevant are, in lieu of a capital theory, contrived. Only by basing such discussions on some coherent theory of capital is it possible to deal in a direct way with the market mechanisms that, potentially, can achieve intertemporal coordination and with policies that may result in intertemporal discoordination.

V. A Summary View

The Austrian theory of the business cycle stands up well to criticism. The integration of monetary theory with a rich theory of capital involving temporally sequenced stages of production coordinated intertemporally by market mechanisms provides a theoretically sound and historically relevant basis for an understanding of the problem of business cycles. Attention to capital theory gives the Austrians a decided advantage over other schools in theorizing about cyclical movements in macroeconomic magnitudes—or more generally, about self-reversing intertemporal market processes.

And as it turns out, the attention to—or neglect of—capital theory serves as well as a peg on which to hang some history of economic thought. Fuller understandings of New Classicism, Monetarism, and different renditions of Keynesianism are made possible by noting how they did or why they did not take capital into account. Except in Keynesian theory, which lacks the very coordinating mechanisms that the Austrians have for so long illuminated, intertemporal coordination gets achieved, however well or badly, in some market process. If the coordinating mechanism does not take the form of an interest rate that determines the intertemporal allocation of capital, then it must take some other form—correctly or incorrectly interpreted price changes that cause individuals to store money or make some adjustment in their consumption behavior; perceived or possibly misperceived wage rates that allocate the employment of labor over time; or investment activities that are governed by the waxing and waning of business confidence.

Austrians are often criticized for placing too much emphasis on or according too much importance to their business-cycle theory. Why all the attention to nineteenth-century business cycles or to the Great Depression, which in so many respects was a unique historic event? While the Austrian theory does have

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a direct applicability to these historical episodes, it has broader significance as well. Austrian capital theory amounts to a theory of intertemporal coordination; Austrian business-cycle theory (that is, the analysis of the effects of an exogenous monetary expansion in the light of Austrian capital theory) amounts to a theory of intertemporal discoordination. And even more broadly, calling attention to the Austrian theory of the business cycle constitutes an appeal to the economics profession to put capital theory back into macroeconomics.

Notes

1. The concept of involuntary unemployment, for instance, is found to be meaningless in the context of New Classicism and its equilibrium models of the business cycle. "In these models the concepts of excess demands and supplies play no observational role and are identified with no observed magnitude" (Lucas [1981, p. 287]). For a critical survey of this and similar aspects of New Classicism, see Yeager (1986, pp. 382–86) and Leijonhufvud (1986, pp. 418–19).

2. Although Rothbard has written extensively on the Austrian theory of the business cycle, Tullock's critique draws exclusively from "Economic Depressions: Their Cause and Cure," most readily accessible in Mises et al. (1983). Tullock references the original publication by Constitutional Alliance, Inc., which he reports as having no publication date. My copy is dated 1969.

3. Leijonhufvud's analysis highlights disturbances and adjustments and downplays the point that the disturbance of the economy from its natural growth path may be either in the positive or the negative direction. If we were to focus on this positive/negative distinction and to divide the adjustment process into an initial phase and a final phase, we could categorize Leijonhufvud's cycles as *X/N* or *N/X*. The first category would include a positive disturbance caused, say, by an increase in the money supply, which is eventually rectified as prices are adjusted upward to accommodate a higher level of nominal cash balances; the second category would include a negative disturbance caused, say, by an increase in money demand, which is eventually rectified as prices are adjusted downward so as to produce a higher level of real cash balances.

4. Tullock sees monetary stimulation as analogous to the stimulation of agriculture brought about by USDA programs and to the stimulation of industry as might be brought about by a system of taxes and subsidies. The analogies fail, however, because of the absence in USDA programs and tax/subsidy schemes of any self-reversal. "Suppose that the government taxed consumer goods and used the money to subsidize investment. Suppose further that after a while it stopped the subsidy" (Tullock [1987, p. 77]). The "Suppose . . ." followed by "Suppose further . . ." is a characteristic of disturbances in the *X/X* category. Hence, distortions caused by such a sequence of fiscal policies are categorically different from distortions caused by monetary stimulation.

5. In defending his own account of the boom-bust sequence against a challenge by John Hicks, Hayek (1978) drew attention to the first and third considerations spelled out here, the first in terms of the absence of perfectly competitive conditions in the

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market for lonable funds and the third in terms of "fluid equilibria." Though responsive to Hicks, whose objections were based on comparative-statics analysis, Hayek overlooked the potentially exacerbating effects of rational expectations that are revealed by comparing the dynamics of the production process to the dynamics of a chain letter.

6. As an alternative illustration of all three considerations, note that when the government discovers a counterfeiting ring, it immediately shuts it down. Does the New Classicist view imply that accurate and timely publication of the total money supply inclusive of the ring's contribution would be an equally effective policy?

7. It is ironic, in view of these empirical roots, that the Austrians are so often accused of having no empirical foundation for their business-cycle theory. What is true is that the Austrians have always rejected the modern positivists' strategy of fabricating wholly abstract models and then mining the available statistical data to determine whether such models may be related in any way to real-world events. For the Austrians, history (which includes but is not limited to statistical data) and theory (which helps to make history intelligible) are complementary disciplines (Mises [1969]).

8. Though concerned with chronic resource idleness rather than with an unsustainable boom, Keynes lamented the loss of commitment brought about by the emergence of organized capital markets. "The spectacle of modern investment markets has sometimes moved me towards the conclusion that to make the purchase of an investment permanent and indissoluble, like marriage, except by reason of death or other grave causes, might be a useful remedy for our contemporary evils" (1964, p. 160).

9. Tullock (1987, p. 78, footnote 8) objects to Rothbard's account of the business cycle on the grounds that roundaboutness in investment occurs in the depreciation of plant and equipment rather than in some more narrowly conceived production process. As an empirical matter, this claim may well be correct. In the earliest modeling attempt by Hayek (1967), the focus was confined to a continuous-input/point-output production process. This construction allowed the highlighting of the time element in the production process without involving the complications of durable capital. Applications of the theory, of course, require that the time element in all its manifestations be taken into account. Tullock is suggesting, in effect, that a model involving depreciating capital goods would be more realistic and hence more directly applicable to actual production processes.

Tullock's claim (p. 76) that "the producer goods industries are always a fairly small part of the economy" is puzzling. Surely, industrial economies are to be distinguished from primitive economies in terms of the size—fairly large and fairly small, respectively—of their producer-goods industries. In any case, the problem of intertemporal coordination can be incorporated into economic theory by distinguishing between relatively more time-consuming and relatively less time-consuming production processes.

10. Keynes appears to be adopting a strategy usually confined to the legal profession: "My client didn't borrow your urn; it was in perfect condition when he returned it; and it was already broken when you lent it to him." Keynes was "arguing like a lawyer" that the profession's attention should be directed away from wage rates and toward interest rates. This view of Keynes is consistent with Leijonhufvud's (1968).

11. An alternative interpretation is that opponents of the Austrian view have pursued, in effect, a divide-and-conquer strategy: sort out that part of the Austrian theory that looks like fixed-capital macroeconomics and pit it against Keynesian theory; sort out that part that looks like the economics of capital accumulation and pit it against

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neoclassical growth theory. Neither part fares well on its own. The key to the divide-and-conquer strategy is the working hypothesis that capital is homogeneous. Lachmann identified this battleground and recognized the significance of the battle more than thirty years ago:

> Once the homogeneity postulate has been abandoned the distinction between growth and fluctuations loses its meaning. The distinction finds a place in a theory which confines itself to asking whether and to what extent existing resources are being used, whether, and perhaps at what speed, such resources can be augmented, and what are the circumstances in which such augmentation is likely to take place. Once we have learnt how to ask how, and in what order, existing resources are being used, and what are the implications of such multiple use, once we have begun to understand the importance of the concrete form of resources in limiting the scope of multiple use, we can easily dispense with the all too simple distinction between economic growth and cyclical fluctuations. (Lachmann [1978, p. 112]).

12. Leijonhufvud (1981b, p. 140ff.) rejects the elasticity-based distinction between Keynesianism and Monetarism: demands for liquid assets and for investment funds are not *always* characterized by some particular elasticity. Each is sometimes elastic, sometimes inelastic—depending on the state of expectations and the circumstances created by the particular policy regime. From an Austrian viewpoint, Leijonhufvud can be faulted only for a sin of omission. Is it not more instructive to call attention first to the intertemporal structure of capital, which is ignored by both the Keynesians and the Monetarists, and then to expectations and policies that influence the allocation of resources within that structure?

13. Some Keynesian scholars will undoubtedly object to the claim that Keynes trivialized the time dimension of the production process. Yet in his discussion of the nature of capital, he suggested that a given process has all sorts of attributes—which include "smelliness" as well as "roundaboutness." No single such attribute, according to Keynes (1964, p. 215), has any special claim on our attention. Keynes's deep-felt concern about "the dark forces of time and ignorance that envelop our future" (p. 155) is expressed several chapters earlier in a discussion of long-term expectations. The order of the two discussions is revealing: Keynes deals with expectations about long-term rates of profit without having brought into view the proximate objects of those expectations—the capital goods that give concreteness to the structure of production.

14. The social losses attributable to monetary disruptions are not at all accurately measured by unemployed labor (Wagner [1979]). Some distortions of the capital structure may involve no unemployed labor at all. Conversely, labor employed to undo such distortions can hardly be counted—in a broader context—as social gains. Tullock's claim (1987, p. 77) that money-induced distortions of the capital structure should, according to his understanding of the Austrian theory, give rise to *higher* living standards simply ignores the intertemporal discoordination identified by the Austrians. It is true that if we reinterpret the theory in the context of a Knightian stock-flow conception of capital, then we can argue that monetary expansion forces individuals to forgo part of the consumption flow in order to add to the capital stock, after which the flow is permanently higher. But such an intertemporal distortion of the flow of output cannot, on the whole, be considered welfare-enhancing.

15. Assertions by Lucas (1981, p. 219) are explicit:

> One exhibits understanding of business cycles by constructing a *model* in the most literal sense: a fully articulated artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies. The Keynesian macroeconomic

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models were the first to attain this level of explicitness and empirical accuracy; by doing so, they altered the meaning of the term *theory* to such an extent that the older business cycle theories could not really be viewed as "theories" at all.

16. Leijonhufvud (1986, p. 417), who claims to have been overexposed to the Austrian theory, rejects that theory on the basis of such simple calculations involving high levels of aggregation: "My trouble with ABC [Austrian business-cycle theory] is that its . . . falsifiable content has been falsified. According to ABC, inflation should produce an overinvestment boom. The stagnation decade of the 1970s does not fit: it gave us inflation but no acceleration of capital accumulation." But see Leijonhufvud (1976) for a more thoughtful discussion of the relationship between theory and evidence.

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A Subjectivist Perspective on the Economics of Crime

Samuel Cameron

Inspired for the most part by the Chicago faction, orthodox economists have embarked on a considerable amount of empire building in the past two decades. They have purported to show the usefulness of their approach to problems such as sex, marriage, divorce, crime, abortion, and church attendance. (See McKenzie and Tullock [1978] and Radnitzky and Bernholz [1987] for textbook demonstrations of this.) On a cynical level, one might regard this empire building as a vehicle for insuring the expansion of the number of Ph.D. topics and publications to satisfy the career needs of the increasing supply of economists. Empire building is particularly suitable for such a purpose as there usually awaits the invader a large body of data that was previously unexplored or "inadequately" explored by other social scientists using tools with less cutting edge than those of the econometrician. Motives may not matter a great deal if the economic approach is in some sense useful in the areas into which it has been extended. This begs the prior question as to whether the orthodox economic approach is not potentially misleading on traditional topics. Subjectivist economists have long contended that it is (Yeager [1987], Bellante [1983]). This is the point of departure for this article as my contention is that the fallacies attendant upon neoclassical hubris have permeated the majority of research on the economics of crime. Not surprisingly, this point has been made previously by sociologists (see the comments in Rottenberg [1973] and also Glaser [1977]) and radical economists such as Gordon (1971), although Gordon simply restates propositions from traditional sociology and criminology that he then designates radical political economy.

This first section sketches the general equilibrium model of crime employed in the literature.[1] The first equation is the "crime-supply" equation, which relates the amount of crime to a set of variables intended to capture the costs and benefits of engaging in criminal activity rather than leisure or legal activity. These variables are of two types. The first set is exogeneous, consisting of items that measure the wealth generated in noncriminal activity and the wealth obtainable from crime if the perpetrator is not captured or punished. The second set

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comprises variables indicative of the certainty and severity of punishment such as arrest (or clear-up) rates, conviction or imprisonment rates, police; manpower, and expenditures. Most studies include some but not all of the punishment variables and treat them as endogenous. Usually the only endogenous variable is the arrest-rate variable. Thus the second equation is usually designed to explain the arrest rate. This is often labelled a police-production function. As police production is heavily labor-intensive, it is really a single-factor police-production function with the output (arrest rates) being related to the volume of police resources. Other exogenous variables are included to allow for differences in the nonlabor constraints across regions. For example, population density may influence the cost of making an arrest ceteris paribus. In some cases, an endogenous variable in the form of the crime rate is included as a "work-load" variable.

The rest of the model concerns the derived demand for instruments of crime prevention/reduction. The usual third equation is a demand-for-police function where policing is treated as an intermediate semipublic good that generates lower crime rates through the second equation. Demand is thus a function of the crime rate, the "price" of a unit of policing, and income. The usual operational model stops here although some consider a fourth equation to take account of the operation of the judicial system. This equation may treat sentencing as a function of the crime rate, constraints of available prison resources, and so on. In neoclassical terms, this might again be termed a production function although this is rather dubious terminology, as will be argued later.

The neoclassical general-equilibrium model recognizes many of the feedbacks between crime and responses to it plus the need to solve the problem in statistical work of identifying the structural equations. In the econometric work, the problem is "solved" by arbitrary exclusion of some exogenous variables from some equations. This weakness need not detract greatly from the apparent attractiveness of the approach for policy making provided we look only at exogenous variables as the impact of these can be assessed without complication through the reduced forms of the model. Policy analysis derives some of its credibility from the underlying theory that has been used to motivate number-crunching exercises. We now look critically at this underlying theory.

Superficially the theory of criminal activity is a subjective one as it derives allegedly from subjective expected-utility theory according to which decision makers choose on the basis of the weighted average utility from success or failure in some act. The weights are the subjective probabilities that the individual attaches to outcomes. In the case of crime, the relevant outcomes are states where some punishment is experienced and the punishment-free state of escape from detection. Following the Chicagoan methodology of Becker and Stigler (1977), all individuals obey the same calculus and even have the same utility function, which is unchanging over the life cycle. Anyone will, in this model,

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become a criminal if the price is right; if the prices cease to be right, people will cease being criminals. Entry and exit into and out of crime are symmetrical. It is thus mistaken to talk about criminals as all individuals have ex ante criminal intentions; whether or not they are observed ex post to perform criminal acts simply depends on the values of the relevant constraints and expectations in the time period under consideration.

There are at least two important subjectivist criticisms of the crime-supply function that come to mind even before we question the validity of the supply-function concept in this context. First, the econometric work and the conclusions drawn from it pay no more than lip service to the notion of subjective expectations. It is hard for the situation to be otherwise as measures of criminal expectations are not used. The probability of being caught is represented by the arrest rate for the crime under consideration. This ratio is simply the total number of arrests divided by the total number of crimes. With a bit of difficulty we might accept this as a measure of the *objective* expectation of capture for the average individual. Even so there is no guarantee of a monotonic stable relationship between the arrest rate and subjective expectations of capture; even if a criminal knows for certain that the aggregate arrest rate has risen, he may not revise his personal capture probability because he does not perceive himself as similar in characteristics to those being arrested. The situation is not analogous to other risk situations such as that of driving along a road that is an accident black spot; all drivers coming on to this from a relatively safe road would perceive themselves as at increased risk. Where we move to a regime of higher arrest rates, some criminals may perceive their own expertise as sufficiently great that they are still at the same risk as before (presumably a very low one). Using a subjectivist perspective, we ought to ask what a rise in arrest rates means to the individual criminal; in part it conveys information that the given stock of criminal methods is less efficient than formally thought and as such it may prompt a change in *modus operandi* to perform the same number of crimes without appreciably greater risk (cp. Cook [1979]).

The second criticism is that the concept of subjectivism in the subjective expected-utility approach is restrictively static. There is no explanation as to where those contemplating crime get their expectations although some empirical work (McPheters and Stronge [1974], Bahl et al. [1978]) grafts on a crude adaptive expectations mechanism without giving any crude rationale for its use.[2] The unsatisfactory treatment of expectations is promoted by the symmetry of entry and exit just discussed. The expectations held by different individuals will depend on their position with respect to the crime process. Someone who has never performed a criminal act in the past can only get his expectations from others who have or from very general sources such as newspaper reports or anecdotal evidence. It is doubtful that very meaningful expectations can be formed from these sources. An individual's first crime is unique to the individual despite the banality of its appearance to some criminologists or

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economists. In some respects, it is an act of investment. If performed with others, the individual may learn crime-relevant techniques "on the job." Regardless of the organizational setup of the crime, the individual will learn about the risks of capture, his own feelings of stress or enjoyment at participating in a high-risk stigmatized activity, methods of escape, and perhaps the problems and returns involved in attempting to dispose of stolen property. Naturally the last of these does not apply to white-collar crime such as computer fraud, although it is replaced by the experience of learning how to dispose of income that others would not expect one to have without drawing attention to it. The preceding experiences cannot usefully be anticipated; it is mainly through participating in an act of crime that expectations can be developed. Vast numbers of people respond to the problem of forming precrime expectations by imagining the worst, viz. that the guilt will be so unbearable that a life of crime will prove intractable and/or that they will be so incompetent that they will be caught. If such extreme risk aversion were widespread in the population and stable over time, there would be no criminals whatsoever. Why then do some people commit crimes? The orthodox answer would rely on rational expectations so that the mean of the distributions of guilt, ability to escape detection, and so on would be decisive. It is hard to see how these distributions can have much meaning for the potential criminal as they relate to those who have already crossed the barrier from perceiving themselves as noncriminal into perceiving themselves as criminals. Either some people simply hold optimistic expectations about becoming criminals or there are some other factors influencing entry to crime.

Optimistic expectations may arise from simple ignorance and/or upbringing in a particular cultural milieu i.e., some individuals may have an excessive confidence in their own ability in untried arts and also downplay the stress of living under the shadow of guilt. In a subjectivist view, it is perhaps too simple to think of there being separate groups of individuals with optimistic or pessimistic expectations. All individuals may continually entertain both scenarios alternately, facing a perpetual need to resolve the conflict. The resolution cannot be made in the way described in orthodox economics as the relevant probabilities can only be known through repeated sampling; it is not possible for an individual to repeatedly sample crime without engaging in some. Once some, even if only one, crime has been sampled, there will be adaptations in the behavior of the individual. Reversion to previous behavior is not simply symmetric. Having once crossed the barrier into crime, one has made some investment in being a criminal in that bearing the burden of guilt might be considered a form of overhead capital[3] that has no alternative use in legitimate markets. Put simply, the difficulty of performing the second crime should be much less than that of the first as stressful imponderables have now entered the realm of the ponderable. This is providing that the first-time offender is not caught. The effect of capture depends on the response of law enforcement agencies. For very young offenders and minor infractions, severe

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warnings without punishment may serve as a deterrent sufficiently powerful to dissuade the offender from ever making another attempt. Publicly visible punishments may serve to enhance the attractiveness of criminal activity to the individual in a number of ways. Empirical workers such as Myers (1983) have noted that the stigma of past convictions and the "training" effects of prison will work to shift the balance of advantages to crime rather than legal activity. We may further note that conviction may help cement an individual's previously uncertain self-image. It might be instructive to liken the situation to that of becoming an entrepreneur. In economic models, the entry to entrepreneurial activity is simply based on comparisons of costs and benefits with alternatives. Becoming an entrepreneur involves much more than this in that it may satisfy an individual's needs for power, exercise of creativity, excitement, social importance, and so on. So it is with crime. In the case of supplying illicit goods and services, the criminal may see himself as socially beneficial in correcting government attempts to subvert the market mechanism. Where the crime involves theft or transfer from the incomes of others, the criminal may perceive himself as more clever than other people. Such activity may be parasitic, if the criminal exploits other individuals who have submitted themselves to the restrictions of market discipline. Punishment may serve to crystallize to the individual that he is acting in the just mentioned roles. In the prepunishment period, an individual may be undecided over his relationship to legal and illegal activity. Punishment modifies perceptions by revealing to the criminal that he is now perceived as a criminal by the rest of society. In short, conviction and punishment may provide the subjective push that is needed to generate the process of a criminal career.

The criminal career involves entry at an early age followed by changes in the volume and type of crime as the criminal matures; eventually individuals may drop out of crime altogether when they age sufficiently (Blumstein and Cohen [1987], Cameron [1987]). Traditionally the criminal career idea has not permeated the work of economists to any great extent. This does not mean that a conventional economic analysis of it is not possible.[4] Such an approach could be developed by taking account of learning and information acquisition. As time passes, a criminal learns about the techniques of crime and the available opportunities for crime from other criminals. Punishment plays a special role in this as the prison is a veritable university of crime where the tyro criminal has an opportunity to learn from the past successes and failures of masters of the art. In the Chicago approach, entrants to crime are rational utility maximizers with time-invariant preferences; this implies that they should purposefully get arrested in order to obtain entry to prison! The subjectivist critique is that this does not happen because, as I pointed out, entry to crime is an act of resolving conflict in an environment where risks are not known because of the absence of repeated sampling and the substantial uniqueness of each person's entry. Mixing with more experienced criminals cements the decision to

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become a criminal, but it is not the by-product of a deliberate decision to obtain human capital by going to prison.

Subjectivist critique can shed some light on correlations that have puzzled criminogenic economists. Although the belief does not automatically follow from orthodox theory, economists have held strong expectations that all punishment and probability of detection variables should deter crime. It has often been found (see Cameron [1988]) that prison correlation may be explained by the "training" effects. The positive correlation with police arises in almost all studies that insert the level of policing directly in the crime-supply equation in place of the arrest rate. Interestingly, Sandelin and Skogh (1986) include the police *and* arrest variables in the supply of property-crime equation. They find that the police variable is negatively related to the crime rates, but the arrest rate is positively related to the crime rate. Their explanation for this contradiction of their expectations is the weak post-hoc one that "criminals react to the size of the police force rather than the arrest rate" (p. 555). This is even self-contradictory as the arrest rate did influence crime rates with a significant positive correlation. It is clear from our earlier discussion that these allegedly puzzling correlations are to be explained in large part by the failure of economists to measure criminal's subjective expectations in the subjective expected-utility model. Further, it is conceivable that high levels of punishment and/or policing generate an increased predisposition to criminal behavior. There are two ways in which this may happen. First, capturing inexperienced criminals at low-skill crimes may lead to greater high-skill crimes in the future when they have accumulated human capital.[5] Second, the frequency of formal contacts with police officers when engaged in acts that may be treated as criminal could tip the scales in favor of a subjective evaluation of oneself as criminal when the conflict over individual identity is being resolved.

An additional difficulty is that the arrest rate is arguably not a very good measure of the objective probability of capture for an individual. Most of the difficulty arises from the fact that arrest rates are not objective measures such as temperature or weight; indeed, they are not even pseudo-objective measures such as gross national product. Rather, they are the outcome of administrative statistics controlled by the police. This is more appropriately dealt with later in the context of police production.

My final comment on the individual supply-of-crime function concerns the problem of whether it is meaningful to conceive of such an entity at all. This has been hinted at in an econometric critique of Ehrlich's work on capital punishment. Forst, Filatov, and Klein (1978, p. 343) comment:

> His theoretical model is much like that used to derive specifications for demand and supply functions in the economic analysis of production and consumption. In that respect, we find his theory strongly contrived, i.e. *set up in an essentially imitative way vis à vis the design of economic theory that*

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> *is used to generate specifications of econometric models. We have doubts about the insight that this approach is likely to bring to an understanding of criminal behavior.* (emphasis added)

Unfortunately the authors of the preceding proposition choose not to expatiate. There is a subtle methodological point concerning the possibility that *all* economic models are really constructed by analogy with markets because the institutional environment of most commodity exchange is not a market in any sense. I shall not pursue this here, but shall concentrate instead on the matter of how much criminal activity departs from other supply behavior such as consumer goods or labor time. The most obvious point of departure is that there is no demand for the supply of criminal effort except in the case of demand derived from the need of some criminals to have others work with them. There is complete market failure in the literal sense that the potential targets of crime do not have an offer curve of how much they are willing to pay for protection from victimization. Even in situations of extortion, the contract is nonenforceable (i.e., the victims have no recourse if the extorters demand a pay raise outside the initial agreement). Given the general absence of direct trade between victims and criminals, the demand has to be derived from the demand for protection against crime. Suppose that all households could at sufficiently low cost procure 100 percent effective protection; then the supply of crime would vanish. As protection is necessarily less than 100 percent effective, there is scope for supply of crime, which should vary in response to the effectiveness of protection. If the supply-function concept is meaningful here, then shifts in the "demand" for crime in the form of locks, burglar alarms, and so on should influence the level of crime through their impact on the expected rate of return. As pointed out in connection with policing, there are formidable difficulties for the criminal in knowing this. Suppose improved locks become general. How are criminals to know this? Only learning by doing (or learning from others doing) will suffice; by the time a criminal has reached the point where he discovers a superior lock in place, he will already have made investments of time, planning, equipment, and so on and taken risks—all of which will come to nothing if he simply gives up at this point. There is then some incentive to alter the planned mode of entry to something that may be riskier and less rewarding but still the source of net gains. This arises because of the inapplicability of competitive supply functions. Crimes are not performed at the margin where the rate of return equals the marginal productivity of the criminal.[6] An increase in the supply of criminals need not decrease the expected rate of return as there will, at any time, be unexploited opportunities for crime at the prevailing rate because of risk. It is hard to visualize the process of competition that would occur in the "crime industry"; either criminals would have to turn up at the same location and fight over it or the more efficient criminals would somehow be able to deter the less efficient from poaching

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on their intended targets. Neither situation seems plausible except for highly organized crimes. We are forced to conclude then that the notion of an aggregate supply function of crime does seem to be constructed by analogy with markets. There do *not* seem to be competitive processes at work in the crime sector. In a trivial sense, we can talk about supply functions in the sense that criminals obviously allocate (supply) some time to crime rather than rival legitimate activities. The existence of a supply and the construction of analogies to prices does not however automatically guarantee that the supply function concept can be legitimately employed.

We now consider the underpinnings of the second equation in the orthodox general equilibrium approach to crime.[7] This equation relates the resources allocated by the community to the police sector. A peak of objectivism is reached here as the usual work bypasses all questions of coordination by the public-sector managers who are analogous to entrepreneurs. The police-sector managers are assumed to passively translate the given inputs into the outputs that the community demand function has stipulated. Under such circumstances, there is a straightforward correspondence between the production and cost functions achieved through objectively given production processes and exogenously set factor prices. The idea of an objective cost function in the private sector has been criticized by Yeager (1987, p. 16) on the grounds that "cost curves are no more objectively given to business firms than are demand curves for their products. A large part of the task of entrepreneurs and managers is to learn what the cost (and demand) curves are." This applies with even greater force to the issue under consideration.

Subjectivism enters the analysis of police production in the shape of the difficulty of identifying police output. Presumably police output is whatever the community demands and uses to monitor police performance. In conventional economic terms, output should represent value added. The econometric work on police-production functions (see Cameron [1987a]) uses arrest or clearance rates as the output measure. It is hard to see this as adding value to anything. It may serve to reduce the loss of value from crime if it acts to deter criminals. Even if this does occur, there are difficulties in translating this into value added for the consumers (potential victims) of police services. In the orthodox approach (see Bartel [1975]) they too would obey the calculus of subjective expected-utility theory. The objections to this approach are the same as those to applying it to the performance of crime; indeed, their force may be even greater. How can a firm or household possibly know the relationship between the level of protection and the probability of victimization? Learning about it through random sampling seems rather difficult. Suppose a householder deliberately varied his level of protection to derive the supply of crime to his property. Over a large range of levels of protection, no crime would be observed at least in part because crime is a relatively rare event. If a crime did occur, how would the householder

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separate the random and systematic elements in its occurrence (i.e., how is he to know that the crime would not have taken place whatever the level of protection)? The learning that may take place from having been victimized may well suffer from built-in obsolescence. If the mode of entry and so on of the criminal lead to improvements in the type of locks and so forth, criminals could well recognize these changes and adjust their mode of entry to render them ineffective. Casual empiricism suggests that value added to the consumers of police "production" is really just reassurance against a feeling of fear (Cameron and Golby [1987]). This is much too vague and subjective to be satisfactorily rendered into the concept of a police-production function.

The problem of identifying and measuring value added from police work brings difficulties for those who actually perform it. Someone involved in a simple manufacturing process can easily see what is expected of them and gauge how well they are achieving it. Further motivation can readily be stimulated by appropriate incentive mechanisms. With police work, the expectations entertained by workers are much more nebulous. Satisfactory service to the community requires entrepreneurial acts of innovation and marketing by the ultimate managers of the police force. They cannot simply sit back and administrate the performance of clearly stipulated activities. It is not clear *even to the workers in the police service* what is the precise functional relationship between any of their acts and the level of crime; indeed, it is unlikely that they perceive that any such relationship exists. The most appropriate concept of police output is as information in the minds of the community. Thus the police force managers may generate an increase in welfare through process innovation such as developing programs of "community policing" or reallocating resources (e.g., putting more officers on patrol in an area perceived as dangerous even though there may not be any actual changes in the victimization probabilities of members of the community). In a restricted sense, we might see this as a conventional response of producers to demand conditions where the product demanded is highly intangible. With an intangible product, the consumer should employ some proxies for the true level of output. Provided that these proxies are sufficiently correlated with "true" output, the conventional analysis should apply. The important difference here is that production deals almost entirely *with the generation of subjective perceptions*, not with products that are viewed differently by different consumers but that still have an objective quantifiability. Consumers are not simply "rationally ignorant" as are voters in the public-finance literature (see Browning and Browning [1987, chapter 3]), who could find out all they want to know about public-sector output but are deterred by prohibitive costs of information gathering and processing. Rather, consumption of police output can never be fully assessed no matter how long or how expensive the search of consumers. To a great extent, this is due to the protean quality of such output; the flow of information from the police administration to the public largely determines the perception of output.

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The management of the police sector also requires attention to police personnel at lower levels of the organization. Satisfaction of community requirements dictates the hiring and retention of officers who will contribute positively to the production of feelings of security. In the conventional approach, the entry to the police sector is again based on subjective expected utility with inidividuals making a career choice based on the discounted expected lifestreams of earnings from different occupations. The pecuniary returns to police work are much more easily estimated than the nonpecuniary. Joining will be influenced by psychic income derived from the components of danger, glamour, social contribution, and so on. In the orthodox approach, these would be convertible to monetary equivalents that would enter into the calculus of subjective expected-utility theory weighted by their perceived probability of occurrence. The pyschic-income components will be valued differently by different individuals according to their personality type. Yet again we have the problem of how a measurement of that which is to be valued is obtained. Individuals cannot sample the nonpecuniary returns of police work without joining. The major alternative source of information is popular-media portrayals of the life of police officers. These give excessive emphasis to the amount of action and excitement involved as well as portraying an image that crimes are continually being solved by the application of thought and bravado. Such distortions serve the function of perpetuating the idea that police inputs do generate tangible outputs. In addition, they help attract individuals in search of an active, socially useful life. A study by Van Maanen (1975) shows that the morale of recruits drops steadily after joining. This may be characterized as learning about the true nature of police output. Bearing in mind that managers will seek to satisfy the need of citizens for reassurance, there is an incentive to motivate lower-level workers to help perpetuate the feeling that "something is being done about crime." They are more likely to perpetuate this feeling the less they themselves question the proposition that something can be done. Avoiding such questioning dictates an emphasis on action and variety rather than appraisal in day-to-day police work. A successful police-sector manager/entrepreneur would therefore divert administrative work such as record keeping to civilians rather than police personnel as a high ratio of time on administrative duties to time on "real" police work will promote poor self-image by officers with a consequent effect on their public profile. A similar process applies to mundane civil offenses. (For example, in England, parking violations were originally part of police work, but were diverted to civilians in uniform in the 1960s.) Again, the manager should allocate his worker's time in a way that makes their job seem more important and relevant. As an example, resources might be moved from less exciting duties to undercover drug investigations. In a neoclassical model, the ratio of these two types of activity would be determined by their exogenously given societal marginal valuations. The marginal valuations are not of course exogenous as they are determined

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by the control exercised by police managers over the flow of information. It follows from all of the preceding that the cost structure of police operations is not given, but is a product of exploratory enterprising behavior by the police-sector managers.

In the general equilibrium model of crime, deterrence is separated into two branches: police (who mostly determine the probability of punishment) and the judicial system (which mostly controls the severity of punishment). We have thus far not discussed the latter. There is little to add by way of a subjective critique. In the majority of papers estimating crime models, sentencing is treated as exogenous (for exceptions, see Carr-Hill and Stern [1979] and Nagin [1981]) although there is a separate literature on the welfare economics of crime that analyzes substitution between punishment probability and severity in the welfare function.[8] The determination of sentences by judges is subject to the same kind of filtering of citizens' subjective perceptions of the costs of crime as goes on in police departments. The major difference is that judges do not influence the flow of information to the citizens. Hence it is difficult to discern elements of entrepreneurship in the judicial role. The appointment of judges is not generally contingent on the accuracy of their estimates of public perceptions; only ludicrous deviations from public sentiment such as letting brutal murderers walk free in situations where there are no mitigating circumstances would be likely to lead to deposition. In situations where they are appointed rather than elected, judges are not in a very strong position to display adaptive behavior as they have little opportunity (unlike the police) to observe the consequences of their actions.

A vast literature exists on the economics of crime, the great bulk of it being constructed by analogy with neoclassical general-equilibrium models of commodity-market and factor-market interdependence. I have appraised this approach in a subjective light. The main conclusion of this look is that for the most part, the economics of crime has been methodologically misguided. From time to time in the literature, observations have been made that show recognition of this, but these have never been integrated in a meaningful way.

Notes

1. For examples of the implementation of the general economic models of crime, see Buck, Hakim, and Spiegel (1985), Avio and Clark (1978), Carr-Hill and Stern (1979), and Ehrlich (1972, 1973, 1975, 1981).

2. A rationale for an adaptive model is provided by Buck, Hakim, and Spiegel (1985).

3. The same kind of argument is given by Gilad, Kaish, and Loeb (1987) as an explanation for the general manifestation of cognitive dissonance in economic decision making.

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4. A treatment on such lines is given by Friedman, Hakim, and Spiegel (1988), who do not answer the question of why rational utility-maximizing criminals do not go to prison on purpose "to learn."

5. Some evidence suggestive of this is presented in Cameron (1987b).

6. This is not often explicitly stated in the preamble to the presentation of regression results, but see the theoretical papers of Usher (1986, 1987), which do make clear this property of neoclassical general-equilibrium crime models.

7. The discussion in this section also draws on the third equation as this makes it easier to express the points being made.

8. The welfare literature is reviewed in McDonald (1987).

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The Hermeneutical Invasion of Philosophy and Economics

Murray N. Rothbard

In recent years, economists have invaded other intellectual disciplines and, in the dubious name of "science," have employed staggeringly oversimplified assumptions in order to make sweeping and provocative conclusions about fields they know very little about. This is a modern form of "economic imperialism" in the realm of the intellect. Almost always, the bias of this economic imperialism has been quantitative and implicitly Benthamite, in which poetry and pushpin are reduced to a single-level, and which amply justifies the gibe of Oscar Wilde about cynics, that they [economists] know the price of everything and the value of nothing. The results of this economic imperialism have been particularly ludicrous in the fields of sex, the family, and education.

So why then does the present author, not a Benthamite, now have the temerity to tackle a field as arcane, abstruse, metaphysical, and seemingly unrelated to economics as hermeneutics? Here my plea is the always legitimate one of self-defense. Discipline after discipline, from literature to political theory to philosophy to history, have been invaded by an arrogant band of hermeneuticians, and now even economics is under assault. Hence, this article is in the nature of a counterattack.

To begin, the dictionary definition of hermeneutics is the age-old discipline of interpreting the Bible. Until the 1920s or 1930s, indeed, hermeneutics was confined to theologians and departments of religion. But things changed with the advent of the murky German doctrines of Martin Heidegger, the founder of modern hermeneutics. With the death of Heidegger, the apostolic succession of head of the hermeneutical movement fell upon his student, Hans-Georg Gadamer, who still wears this mantle.

The greatest success of the hermeneutical movement has been achieved in recent decades, beginning in the closely related movement of "deconstructionism" in literary criticism. Headed by the French theorists Michel Foucault, Paul Ricoeur, and Jacques Derrida, deconstructionism in the Western Hemisphere

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is led by the formidable Department of English at Yale University, from which it has spread to conquer most of the English-literature departments in the United States and Canada. The essential message of deconstructionism and hermeneutics can be variously summed up as nihilism, relativism, and solipsism. That is, either there is no objective truth or, if there is, we can never discover it. With each person being bound to his own subjective views, feelings, history, and so on, there is no method of discovering objective truth. In literature, the most elemental procedure of literary criticism (that is, trying to figure out what a given author meant to say) becomes impossible. Communication between writer and reader similarly becomes hopeless; furthermore, not only can no reader ever figure out what an author meant to say, but even the author does not know or understand what he himself meant to say, so fragmented, confused, and driven is each particular individual. So, since it is impossible to figure out what Shakespeare, Conrad, Plato, Aristotle, or Machiavelli meant, what becomes the point of either reading or writing literary or philosophical criticism?

It is an interesting question, one that the deconstructionists and other hermeneuticians have of course not been able to answer. By their own avowed declaration, it is impossible for deconstructionists to understand literary texts or, for example, for Gadamer to understand Aristotle, upon whom he has nevertheless written at enormous length. As the English philosopher Jonathan Barnes has pointed out in his brilliant and witty critique of hermeneutics, Gadamer, not having anything to say about Aristotle or his works, is reduced to reporting his own subjective musings—a sort of lengthy account of "what Aristotle means to me."[1] Setting aside the hermeneutical problem of whether or not Gadamer can *know* even what Aristotle means to him, we push back the problem another notch. Namely, why in the world should anyone but Gadamer, except possibly his mother or wife, be in the least interested in the question of what Aristotle means to him? And even in the improbable event that we *were* interested in this earth-shattering question, we would in any case be prevented on hermeneutical principles from understanding Gadamer's answer.

Deconstruction and hermeneutics are clearly self-refuting on many levels. If we cannot understand the meaning of any texts, then why are we bothering with trying to understand or to take seriously the works or doctrines of authors who aggressively proclaim their own incomprehensibility?

Incomprehensibility

Indeed, a crucial point about the hermeneuticians is that, for them, incomprehensibility is a self-fulfilling prophecy. As a colleague of mine ruefully told me: "I have read everything on hermeneutics I can lay my hands on, and I understand no more about it than I did when I first started." Even in a profession—philosophy—not exactly famous for its sparkle or lucidity, one of

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the most remarkable qualities of the hermeneuticians is their horrendous and incomparably murky style. Stalactites and stalagmites of jargon words are piled upon each other in a veritable kitchen midden of stupefying and meaningless prose. Hermeneuticians seem to be incapable of writing a clear English, or indeed a clear German sentence. Critics of hermeneutics—such as Jonathan Barnes or David Gordon[2]—are understandably moved to satire, to stating or quoting hermeneutical tracts and then "translating" them into simple English, where invariably they are revealed as either banal or idiotic.

At first, I thought that these German hermeneuticians were simply ill served by their translators into English. But my German friends assure me that Heidegger, Gadamer et al. are equally unintelligible in the original. Indeed, in a recently translated essay, Eric Voegelin, a philosopher not normally given to scintillating wit, was moved to ridicule Heidegger's language. Referring to Heidegger's masterwork, *Sein und Zeit* (Being and Time), Voegelin refers to the meaningless but insistent repetition of a veritable philosophical dictionary of phrases as the *Anwesen des Answesenden* ("the presence of that which is present"), the *Dingen des Dings* ("the thinging of the thing"), the *Nichten des Nichts* ("the nothinging of the nothing"), and finally to the *zeigenden Zeichen des Zeigzeugs* ("the pointing sign of the pointing implement"), all of which is designed, says Voegelin, to whip up the reader "into a reality-withdrawing state of linguistic delirium."[3]

On Gadamer and the hermeneuticians, Jonathan Barnes writes:

> What, then, are the characteristic features of hermeneutical philosophy? Its enemies will wade in with adjectives like *empty, vapid, dreamy, woolly, rhetorical*. Gadamer himself tells an uncharacteristic story. At the end of a seminar on Cajetan, Heidegger once startled his devoted audience by posing the question: "What is being?" "We sat there staring and shaking our heads over the absurdity of the question." Quite right too, say the enemies of hermeneutics: the question is perfectly absurd. But Gadamer has only a frail sense of the absurd, and his own readers ought to react as he once—but alas, only once—reacted to Heidegger.

Barnes goes on to say that Gadamer admits "that his thought has sometimes been less than pellucid." He further quotes Gadamer as saying:

> Certainly I sometimes spoke over [my pupils'] heads and put too many complications into my train of thought. Even earlier my friends had invented a new scientific measure, the "Gad," which designated a settled measure of unnecessary complications.

Barnes adds that

> Some may prefer to this self-congratulatory little story a remark which Gadamer makes of his younger self: "Despite my title of doctor, I was still a 22-year

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> old boy who thought rather murkily, who reacted portentously to murky thinking, and who still did not really know what was going on."

Barnes adds: "Did the boy ever grow up?"[4]

At this point we may cite Sir Karl Popper on G.W.F. Hegel, who counts along with Friedrich Schleiermacher as at least a great-grandfather of hermeneutics. What Popper lacks in satiric gifts he makes up in the vehemence of the scorn that he heaps upon the legion of his philosophical enemies, real or imagined. After denouncing Hegel's "high-flown gibberish" and "imbecile fancies," Popper quotes with obvious relish the attack on Hegel by his contemporary Schopenhauer as

> a flat-headed, insipid, nauseating, illiterate charlatan, who reached the pinnacle of audacity in scribbling together and dishing up the craziest mystifying nonsense. This nonsense has been noisily proclaimed as immortal wisdom by mercenary followers and readily accepted as such by all fools, who thus joined into as perfect a chorus of admiration as had ever been heard before.[5]

Why this enormous acclaim and influence exerted by mystifying nonsense? In addition to noting its establishment in the interests of the Prussian state, Popper offers the following explanation:

> For some reason, philosophers have kept around themselves, even in our day, something of the atmosphere of the magician. Philosophy is considered a strange and abstruse kind of thing, dealing with those mysteries with which religion deals, but not in a way which can be "revealed unto babes" or to common people; it is considered to be too profound for that, and to be the religion and theology of the intellectuals, of the learned and wise.[6]

For a final citation on the incomprehensibility of hermeneutics, let us turn to the witty and devastating demolition by H.L. Mencken of Thorstein Veblen, another early protohermeneutician and an institutionalist opponent of the idea of economic law. In the course of an essay featuring the "translation" into English of Veblen's indecipherable prose, Mencken wrote that what was truly remarkable about Veblen's ideas

> was the astoundingly grandiose and rococo manner of their statement, the almost unbelievable tediousness and flatulence of the gifted headmaster's prose, his unprecedented talent for saying nothing in an august and heroic manner. . . .
>
> Marx, I daresay, had said a good deal of it long before him, and what Marx overlooked had been said over and over again by his heirs and assigns. But Marx, at this business, labored under a technical handicap; he wrote in German, a language he actually understood. Prof. Veblen submitted himself to no such disadvantage. Though born, I believe, in these States, and resident here all his life, he achieved the effect, perhaps without employing the means,

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> of thinking in some unearthly foreign language—say Swahili, Sumerian or Old Bulgarian—and then painfully clawing his thoughts into a copious and uncertain but book-learned English. The result was a style that affected the higher cerebral centers like a constant roll of subway expresses. The second result was a sort of bewildered numbness of the senses, as before some fabulous and unearthly marvel. And the third result, if I make no mistake, was the celebrity of the professor as a Great Thinker.[7]

Collectivism

Marx, in fact, has been hailed by the hermeneuticians as one of the grandfathers of the movement. In 1985, for example, at the annual meeting of the Western Political Science Association in Las Vegas, virtually every paper offered in political theory was a hermeneutical one. A paradigmatic title would be "Political Life as a Text: Hermeneutics and Interpretation in Marx, Heidegger, Gadamer, and Foucault." (Substitute freely such names as Ricoeur and Derrida, with an occasional bow to Habermas.)

I do not believe it an accident that Karl Marx is considered one of the great hermeneuticians. This century has seen a series of devastating setbacks to Marxism, to its pretentions to "scientific truth," and to its theoretical propositions as well as to its empirical assertions and predictions. If Marxism has been riddled both in theory and in practice, then what can Marxian cultists fall back on? It seems to me that hermeneutics fits very well into an era that we might, following a Marxian gambit about capitalism, call "late Marxism" or Marxism-in-decline. Marxism is not true and is not science, but so what? The hermeneuticians tell us that nothing is objectively true, and therefore that all views and propositions are subjective, relative to the whims and feelings of each individual. So why should Marxian yearnings not be equally as valid as anyone else's? By the way of hermeneutics, these yearnings cannot be subject to refutation. And since there is no objective reality, and since reality is created by every man's subjective interpretations, then all social problems reduce to personal and nonrational tastes. If, then, heremeneutical Marxists find capitalism ugly and unlovely, and they find socialism beautiful, why should they not attempt to put their personal esthetic preferences into action? If they feel that socialism is beautiful, what can stop them, especially since there are no laws of economics or truths of political philosophy to place obstacles in their path?

It is no accident that, with the exception of a handful of contemporary economists—who will be treated further later—every single hermeneutician, past and present, has been an avowed collectivist, either of the left- or right-wing variety, and sometimes veering from one collectivism to another in accordance with the realities of power. Marx, Veblen, Schmoller, and the German Historical school are well known. As for the modern hermeneuticians,

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Heidegger found it all too easy to become an enthusiastic Nazi once the Nazi regime had been established. And Gadamer had no difficulty whatever adapting either to the Nazi regime (where he was known for having only a "loose sympathy" with the Third Reich) or to the Soviet occupation in East Germany (where, in his own words, he won "the special esteem of the Russian cultural authorities" for carrying out "their directives exactly, even against my own convictions").[8]

"Openness" and Keeping the "Conversation" Going

Here we must note two variants of the common hermeneutical theme. On the one hand are the candid relativists and nihilists, who assert, with an inconsistently absolutist fervor, that there is no truth. These hold with the notorious dictum of the epistemological anarchist Paul Feyerabend that "anything goes." Anything, be it astronomy or astrology, is of equal validity or, rather, equal invalidity. The one possible virtue of the "anything goes" doctrine is that at least everyone can abandon the scientific or philosophic enterprise and go fishing or get drunk. This virtue, however, is rejected by the mainstream hermeneuticians, because it would put an end to their beloved and interminable "conversation." In short, the mainstream hermeneuticians do not like the "anything goes" dictum because, instead of being epistemological anarchists, they are epistemological pests. They insist that even though it is impossible to arrive at objective truth or indeed even to understand other theorists or scientists, that we all still have a deep moral obligation to engage in an endless dialogue or, as they call it, "conversation" to try to arrive at some sort of fleeting quasi truth. To the hermeneutician, truth is the shifting sands of subjective relativism, based on an ephemeral "consensus" of the subjective minds engaging in the endless conversation. But the worst thing is that the hermeneuticians assert that there is no objective way, whether by empirical observation or logical reasoning, to provide any criteria for such a consensus. Since there are no rational criteria for agreement, any consensus is necessarily arbitrary, based on God knows what—personal whim, charisma of one or more of the conversationalists, or perhaps sheer power and intimidation. Since there is no criterion, the consensus is subject to instant and rapid change, depending on the arbitrary mind-set of the participants or, of course, a change in the people constituting the eternal conversation.

A new group of hermeneutical economists, eager to find some criteria for consensus, have latched onto a Gestalt-like phrase of the late economist Fritz Machlup, perhaps taking his name very much in vain. They call this criterion the "Aha! principle," meaning that the truth of a proposition is based on the exclamation of "Aha!" that the proposition may arouse in someone's breast. As Don Lavoie and Jack High put it: "We know a good explanation when

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we see one, and when it induces us to say ahah."[9] Somehow I do not find this criterion for truth, or even for consensus, very convincing. For example, many of us would find the prospect of being confronted with the option of engaging in endless and necessarily fruitless conversation with people unable to write a clear sentence or express a clear thought to be the moral equivalent of Sartre's *No Exit*. Furthermore, I have a hunch that if someone came up with the proposition: "It would be a great thing to give these guys a dose of objective reality over the head" or at the very least to slam the door on their conversation, that this would elicit many more fervent "Ahas!" than the murky propositions of the hermeneuticians themselves.

The prime moral duty proclaimed by the hermeneuticians is that we must at all times keep the *conversation* going. Since this duty is implicit, it is never openly defended, and so we fail to be instructed why it is our moral obligation to sustain a process that yields such puny and ephemeral results. In keeping with this alleged virtue, the hermeneuticians are fervently and dogmatically opposed to "dogmatism" and they proclaim the supreme importance of remaining endlessly "open" to everyone in the dialogue. Gadamer has proclaimed that the highest principle of hermeneutic philosophy is "holding oneself open in a conversation," which means always recognizing "in advance, the possible correctness, even the superiority of the conversation partner's position." But, as Barnes points out, it is one thing to be modestly skeptical of one's own position; it is quite another to refuse to dismiss *any other* position as false or mischievous. Barnes points out that the modest skeptic

> recognizes that he himself may always be wrong. Gadamer's "open" philosopher allows that his opponent may always be right. A modest sceptic may . . . indeed, in his modest way, regard the history of philosophy as a ceaseless campaign, marked by frequent defeats and occasional triumphs, against the ever powerful forces of fallacy and falsehood. . . . [W]ith some opponents he will not be "open": he will be quite sure that they are wrong.[10]

The most important hermeneutical philosopher in the United States is Richard Rorty, who, in his celebrated book, *Philosophy and the Mirror of Nature*, devotes considerable space to the prime importance of "keeping the conversation going." In his sparkling critique of Rorty, Henry Veatch points out that, to the crucial question of how can we conversationalists ever know which ideals or "cultural posits" (in the Rortian language) are better than others, "Rorty could only answer that, of course, there can't be any such thing as *knowledge* in regard to matters such as these." So, if there is no knowledge and, hence, no objective criteria for arriving at positions, we must conclude, in the words of Veatch, that "although Aristotle may well have taught that 'philosophy begins in wonder', . . . present-day philosophy can only end in a total conceptual or intellectual permissiveness."[11] In short, we end with the

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Feyerabendian "anything goes" or, to use the admiring phrase of Arthur Danto in his summary of Nietzsche, that "everything is possible."[12] Or, in a word, total "openness."

But if all things are open, and there are no criteria to guide conversationalists to any conclusions, how will such conclusions be made? It seems to me, following Veatch, that these decisions will be made by those with the superior Will-to-Power. And so it is not a coincidence that leading hermeneuticians have found themselves flexible and "open" in response to the stern demands of state power. After all, if Stalin, Hitler, or Pol Pot enters the "conversational" circle, they cannot be rejected out of hand, for they too may offer a superior way to consensus. If nothing is wrong and all things are open, what else can we expect? And who knows, even these rulers may decide, in a sardonic burst of Marcusean "repressive tolerance," to keep some sort of Orwellian "conversation" going in the midst of a universal gulag.

In all the blather about openness, I am reminded of a lecture delivered by Professor Marjorie Hope Nicholson at Columbia University in 1942. In a critique of the concept of the open mind, she warned: "Don't let your mind be so open that everything going into it falls through."

There is another self-serving aspect to the hermeneutical demands for universal openness. For if nothing—no position, no doctrine—can be dismissed outright as false or mischievous or as blithering nonsense, then they too, our hermeneuticians, must be spared such rude dismissal. Keeping the conversation going at all costs means that these people must eternally be included. And that is perhaps the most unkindest cut of all.

If one reads the hermeneuticians, furthermore, it becomes all too clear that typically no one sentence follows from any other sentence. In other words, not only is the style abominable, but there is no reasoning in support of the conclusions. Since logic or reasoning are not considered valid by the hermeneuticians, this procedure is not surprising. Instead, for reasoning the hermeneuticians substitute dozens or scores of books, which are cited, very broadly, in virtually every paragraph. To support their statements, the hermeneuticians will list repeatedly every book that might possibly or remotely relate to the topic. In short, their only argument is from authority, an ancient philosophic fallacy which they seem to have triumphantly revived. For indeed, if there is no truth of reality, if for logic or experience, we must substitute a fleeting consensus of the subjective whims, feelings, or power plays of the various conversationalists, then what else is there but to muster as many conversationalists as possible as your supposed authorities?[13]

Armed with their special method, the hermeneuticians are therefore able to dismiss all attacks upon themselves, no matter how perceptive or penetrating, as "unscholarly." This lofty rebuttal stems from their unique definition of *scholarly*, which for them means ponderous and obscurantist verbiage surrounded by a thicket of broad citations to largely irrelevant books and articles.

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So why then have not the distinguished critics of hermeneutics played the game on their opponents' own turf and waded through the mountains and oceans of hogwash, patiently to cite and refute the hermeneuticians point by point and journal article by journal article? To ask that question is virtually to answer it. In fact, we have asked some of the critics this question, and they immediately responded in a heartfelt manner that they do not propose to dedicate the rest of their lives to wading through this miasma of balderdash. Moreover, to do so, to play by the hermeneuticians' own rules, would be to grant them too much honor. It would wrongfully imply that they are indeed worthy participants in our conversation. What they deserve instead is scorn and dismissal. Unfortunately, they do not often receive such treatment in a world in which all too many intellectuals seem to have lost their built-in ability to detect pretentious claptrap.[14]

Hermeneutical Economics

Economists like to think of their discipline as the "hardest" of the social sciences, and so it is no surprise that hermeneutics—though having conquered the field of literature and made severe inroads into philosophy, political thought, and history—has yet made very little dent in economics. But the economics discipline has been in a state of methodological confusion for over a decade, and in this crisis situation minority methodologies, now including hermeneutics, have begun to offer their wares.

In the economics profession, of course, the practitioners down in the trenches only loosely reflect, or indeed have scarcely any interest in, the small number of methodological reflections in the upper stories of the ivory tower. But these seemingly remote philosophical musings do have an important long-run influence on the guiding theories and directions of the discipline. For approximately two decades, Lionel Robbins' justly famous *The Nature and Significance of Economic Science* was the guiding methodological work of the profession, presenting a watered-down version of the praxeological method of Ludwig von Mises. Robbins had studied at Mises' famous *privatseminar* at Vienna, and his first edition (1932) stressed economics as a deductive discipline based on the logical implications of the universal facts of human action (e.g., that human beings try to achieve goals by using necessarily scarce means). In Robbins' more widely known second edition (1935), the Misesian influence was watered down a bit further, coupled with intimations no bigger than a man's hand of the neo-classical formalism that would hit the profession about the time of World War II.[15] After the war, the older economics was inundated by an emerging formalistic and mathematical neo-classical synthesis, of Walrasian equations covering microeconomics and Keynesian geometry taking care of macro.

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Aiding and abetting the conquest of economics by the new neo-classical synthesis was the celebrated article by Milton Friedman in 1953, "The Methodology of Positive Economics," which quickly swept the board, sending Robbins' *Nature and Significance* unceremoniously into the dustbin of history.[16] For three decades, secure and unchallenged, the Friedman article remained virtually the only written portrayal of official methodology for modern economics.

It should be noted that, as in the triumph of the Keynesian revolution and many other conquests by various schools of economics, the Friedman article did not win the hearts and minds of economists in the pattern of what we might call the Whig theory of the history of science: by patient refutation of competing or prevailing doctrines. As in the case of the Mises-Hayek business-cycle theory dominant before Keynes' *General Theory*, the Robbins book was not refuted; it was simply passed over and forgotten. Here the Thomas Kuhn theory of successive paradigms is accurate on the sociology or process of economic thought, deplorable as it might be as a prescription for the development of a science. Too often in philosophy or the social sciences, schools of thought have succeeded each other as whim or fashion, much as one style of ladies' hemlines has succeeded another. Of course, in economics as in other sciences of human action, more sinister forces, such as politics and the drive for power, often deliberately skew the whims of fashion in their own behalf.

What Milton Friedman did was to import into economics the doctrine that had dominated philosophy for over a decade, namely logical positivism. Ironically, Friedman imported logical positivism at just about the time when its iron control over the philosophical profession in the United States had already passed its peak. For three decades, we have had to endure the smug insistence on the vital importance of empirical testing of deductions from hypotheses as a justification for the prevalence of econometric models and forecasting, as well as a universal excuse for theory being grounded on admittedly false and wildly unrealistic hypotheses. For neo-classical economic theory clearly rests on absurdly unrealistic assumptions, such as perfect knowledge; the continuing existence of a general equilibrium with no profits, no losses, and no uncertainty; and human action being encompassed by the use of calculus that assumes infinitesimally tiny changes in our perceptions and choices.

In short, this formidable apparatus of neo-classical mathematical economic theory and econometric models, all rests, from the Misesian point of view, upon the treacherous quicksand of false and even absurd assumptions. This Austrian charge of falsity and unreality, if noticed at all, was for decades loftily rebutted by pointing to Friedman's article and asserting that falsity of assumptions and premises do not matter, so long as the theory "predicts" properly. In its founding years in the early 1930s, the Econometric Society emblazoned on its escutcheon the motto, "Science is prediction," and this was the essence of the Friedman-derived defense of neoclassical theory. Austrians such as Mises

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and Hayek replied that the disciplines of human action are not like the physical sciences. In human affairs, there are no laboratories where variables can be controlled and theories tested, while (unlike the physical sciences) there are no quantitative constants in a world where there is consciousness, freedom of will, and freedom to adopt values and goals and then to change them. These Austrian contentions were dismissed by neoclassicals as simply posing a greater degree of difficulty in arriving at the human sciences, but not in offering a troublesome difference in kind.

The neoclassical synthesis, however, began, in the early 1970s, to lose its power either to understand or to predict what was going on in the economy. The inflationary recession that first appeared dramatically in the 1973–74 contraction put an end to a thirty-five–year period of arrogant and unquestioned hegemony by the Keynesian wing of the neoclassical synthesis. For Keynesian theory and policy rested on the crucial assumption that inflationary recession simply cannot happen. At that point, Friedmanite monetarism came to the fore, but monetarism has now come a cropper after making a rapid series of disastrously wrong predictions from the beginning of the Reagan era until the present. But he who lives by prediction is destined to die by prediction.

In addition to these failures of Keynesianism and monetarism, the blunders and errors of econometric forecasting have become too notorious to ignore, and a wealthy and supremely arrogant profession, using ever higher-speed computer models, seems to enjoy less and less ability to forecast even the immediate future. Even governments, despite the assiduous attention and aid of top neoclassical economists and forecasters, seem to have great difficulties in forecasting *their own* spending, much less their own incomes, let alone the incomes or spending of anyone else.

Amid these failures, there has been a chipping away at the neo-classical formalism of Walrasian microeconomics, sometimes by disillusioned leaders operating from within this ruling paradigm.

As a result of these problems and failures, the last ten or fifteen years has seen the development of a classic Kuhnian "crisis situation" in the field of economics. As the positivist neo-classical orthodoxy begins to crumble, competing paradigms have emerged. Sparked also by Hayek's receipt of a Nobel Prize in 1974, Austrian or Misesian economics has enjoyed a revival since then, with numerous Austrians teaching in colleges in the United States and Britain. Recently there have even emerged five or six Austrian graduate programs or centers in the United States.

In a crisis situation, of course, the bad jostles the good in the new atmosphere of epistemological and substantive diversity. No one ever guaranteed that if a hundred flowers should bloom, that they would all be passing fair. On the left, the nontheory of institutionalism has made a bit of a comeback, jostled by "post-Keynesians" (inspired by Joan Robinson) and "humanistic" neo-Marxists who have substituted a vague adherence to "decentralization" and

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protection of all animal and vegetable life forms for the rigors of the labor theory of value.

Which brings us back to hermeneutics. For in this sort of atmosphere, even the underworld of hermeneutics will vie for its day in the sun. Probably the most prominent hermeneutical economist in the United States is Donald McCloskey, who calls his viewpoint "rhetoric" and whose attack on truth occurs in the name of rhetoric and of the eternal hermeneutical conversation.[17] McCloskey, unfortunately, follows the modern path of rhetoric run hog-wild and divorced from a firm anchor in truth, overlooking the Aristotelian tradition of "noble rhetoric" as the most efficient way of persuading people of correct and true propositions. For Aristotelians, it is only "base" rhetoric that is divorced from true principles.[18] McCloskey is now organizing a center for rhetorical studies at the University of Iowa, which will organize volumes on rhetoric in a number of diverse disciplines.

Much as I deplore hermeneutics, I have a certain amount of sympathy for McCloskey, an economic historian who endured years as a drill instructor and cadre leader in the Friedman-Stigler Chicago School's positivist ranks. McCloskey is reacting against decades of arrogant positivist hegemony, of an alleged "testing" of economic theory that never really takes place, and of lofty statements by positivists that "I do not understand what you *mean*," when they know darn well what you mean but disagree with it, and who use their narrow criteria of meaning to dismiss your argument. In this way, the positivists for a long while were able to read virtually all important philosophical questions out of court and consign them to the despised departments of religion and belles lettres. In a sense, the rise of hermeneutics is those departments' revenge, retorting to the positivists that if "science" is only the quantitative and the "testable," then we shall swamp you with stuff that is *really* meaningless.

It is more difficult to excuse the path travelled by the major group of hermeneuticians in economics, a cluster of renegade Austrians and ex-Misesians gathered in the Center for Market Processes at George Mason University. The spiritual head of this groupuscule, Don Lavoie, has reached the pinnacle of having his photograph printed in his magazine *Market Process* talking to the great Gadamer.[19] Lavoie has organized a Society for Interpretive Economics (*interpretation* is a code word for hermeneutics) to spread the new gospel, and has had the effrontery to deliver a paper entitled "Mises and Gadamer on Theory and History," which, as a colleague of mine has suggested, is the moral equivalent of my writing a paper entitled "Lavoie and Hitler on the Nature of Freedom."

It must be noted that nihilism had seeped into current Austrian thought before Lavoie and his colleagues at the Center for Market Processes embraced it with such enthusiasm. It began when Ludwig M. Lachmann, who had been a disciple of Hayek in England in the 1930s and who had written a competent Austrian work entitled *Capital and Its Structure* in the 1950s, was suddenly

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converted by the methodology of the English economist George Shackle during the 1960s.[20] Since the mid-1970s, Lachmann, teaching part of every year at New York University, has engaged in a crusade to bring the blessings of randomness and abandonment of theory to Austrian economics. When Lavoie and his colleagues discovered Heidegger and Gadamer, Lachmann embraced the new creed at the 1986 first annual (and, if luck is with us, the last annual) conference of the Society of Interpretive Economics at George Mason University. The genuine Misesian creed, however, still flourishes at the Ludwig von Mises Institute at Auburn University and Washington, D.C., and in its publications: *Free Market,* the *Austrian Economics Newsletter*, and the *Review of Austrian Economics*, which in its first issue included a critique of a quasi-hermeneutical book by two ex-Misesians who claim to have discovered the key to economics in the works of Henri Bergson.[21]

One of the main motivations of the ex-Misesian hermeneuticians is that their horror of mathematics, to which they react as to the head of Medusa, leads them to embrace virtually any ally in their struggle against positivism and neo-classical formalism. And so they find that, lo and behold, institutionalists, Marxists, and hermeneuticians have very little use for mathematics either. But before they totally embrace the desperate creed that the enemy of my enemy is necessarily my friend, our Market Process hermeneuticians should be warned that there may be worse things in this world than mathematics or even positivism. And second, that in addition to Nazism or Marxism, one of these things may be hermeneutics.

And just as Professor McCloskey's history may serve as a partial mitigation of his embrace of hermeneutics, we may go further back and mitigate the sins of the logical positivists. For, after all, the positivists, much as they may be reluctant to admit it, also did not descend upon us from Mount Olympus. They grew up in old Vienna, and they found themselves in a Germanic world dominated by protohermeneutical creeds such as Hegelianism as well as by the young Heidegger, who was even then making his mark. After reading and listening to dialectics and protohermeneutics day in and day out, after being immersed for years in the gibberish that they were told constituted philosophy, is it any wonder that they—including for our purposes Popper as well as Carnap, Reichenbach, Schlick, et al.—should finally lash out and exclaim that the whole thing was meaningless or that they should cry out for precision and clarity in language? Is it also any wonder that the nascent positivists, like McCloskey a half-century later, should go too far and throw out the philosophic baby with the neo-Hegelian bathwater?

In the peroration to his paean to hermeneutical economics, ex-Misesian Richard Ebeling proclaims: "Man loves to talk about himself."[22] But in rebuttal I point to the sage words of the American cultural and political satirist Tom Lehrer. In the 1960s, Lehrer noted that "a lot of people are whining about their 'inability to communicate.' " "It seems to me," Lehrer added, "that if you

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are unable to communicate, the *least* you can do is to shut up." That, alas, is something that Ebeling and his hermeneutical colleagues have not yet learned to do.

Notes

1. Jonathan Barnes, "A Kind of Integrity: Review of Hans-Georg Gadamer, *Philosophical Apprenticeships* (Cambridge, Mass.: MIT Press, 1985), and Gadamer, *The Idea of the Good in Platonic-Aristotelian Philosophy* (New Haven, Conn.: Yale University Press, 1986)," *London Review of Books* (November 6, 1986), pp. 12–13.

2. Barnes, "A Kind of Integrity"; and David Gordon, "Hermeneutics versus Austrian Economics" (Auburn, Ala.: The Ludwig von Mises Institute, 1986).

3. Eric Voegelin, "The German University and the Order of German Society: A Reconsideration of the Nazi Era," *Intercollegiate Review,* 20 (spring/summer 1985), p. 11.

4. Barnes, "A Kind of Integrity," p. 13.

5. Karl R. Popper, *The Open Society and its Enemies,* 4th ed. (New York: Harper & Row, 1962), vol. 2, p. 33.

6. Popper, *Open Society,* vol. 2, p. 30.

7. H.L. Mencken, "Professor Veblen," *A Mencken Chrestomathy* (New York: Alfred A. Knopf, 1949), p. 270.

8. Barnes, "A Kind of Integrity," p. 12.

9. Don Lavoie and Jack High, "Interpretation and the Costs of Formalism" (unpublished manuscript), p. 14.

10. Barnes, "A Kind of Integrity," p. 13. For a critique of the triumph of the ideal of "openness," see Allan Bloom, *The Closing of the American Mind* (New York: Simon and Schuster, 1987).

11. Henry Veatch, "Deconstruction in Philosophy: Has Rorty Made It the Denouement of Contemporary Analytical Philosophy?" *Review of Metaphysics,* 39 (December 1985), pp. 313–314, 316.

12. Arthur C. Danto, *Nietzsche as Philosopher* (New York: Columbia University Press, 1980), p. 12; cited in Veatch, "Deconstruction," p. 312.

13. I am indebted for this point to Sheldon Richman of the Institute of Humane Studies at George Mason University.

14. In a witty and perceptive article, the distinguished Yale philosopher Harry Frankfurt calls this phenomenon "bullshit," which he asserts to be a greater enemy to the truth than an outright lie, since a liar recognizes that he is violating the truth, whereas the bullshitter does not. Frankfurt writes:

> The contemporary proliferation of bullshit also has deeper sources, in various forms of skepticism which deny that we can have any reliable access to an objective reality and which therefore reject the possibility of knowing how things truly are. These "antirealist" doctrines undermine confidence in the value of disinterested efforts to determine what is true and what is false, and even in the intelligibility of the notion of objective inquiry."

See Harry Frankfurt, "On Bullshit," *Raritan,* 6 (fall 1986), pp. 99–100.

15. Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* [1932] (London: Macmillan, 1935).

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16. Milton Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953).

17. Donald N. McCloskey, *The Rhetoric of Economics* (Madison, Wisc.: University of Wisconsin Press, 1985).

For a comprehensive Misesian critique of McCloskey's work, see the book review essay by Hans-Hermann Hoppe in this volume.

18. Cf. Richard M. Weaver, *The Ethics of Rhetoric* (Chicago: University of Chicago Press) and Larry Arnhart, *Aristotle on Political Reasoning: A Commentary on "The Rhetoric"* (Dekalb, Ill.: Northern Illinois University Press, 1981).

19. *Market Process,* 4 (fall 1986), p. 16.

20. Ludwig M. Lachmann, *Capital and Its Structure* (London: London School of Economics, 1956). The later, post-Shackelian or nihilist Lachmann may be found in his "From Mises to Shackle: An Essay on Austrian Economics and the Kaleidic Society," *Journal of Economic Literature,* 54 (1976).

21. Thus, see Charles W. Baird, "The Economics of Time and Ignorance: A Review," *The Review of Austrian Economics,* I (1987), pp. 189–223.

22. Richard M. Ebeling, "Hermeneutics and the Interpretive Element in the Analysis of the Market Process." Center for Market Processes Working Paper. (Fairfax, Va.: Department of Economics, George Mason University, 1985), p. 45. Cf. Frankfurt, "On Bullshit," p. 100.

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Antitrust Reform: Predatory Practices and the Competitive Process

Dominick T. Armentano

Antitrust Reform in the 1980s

There have been some modest changes in the administration of United States antitrust policy over the past ten years. The federal antitrust regulators—and to some extent the courts—are now somewhat less concerned than previously with conglomerate and vertical mergers; with horizontal mergers within liberalized merger guidelines; with tying agreements that intend to limit "free-riding" or reduce transactions costs; with market concentration achieved through internal growth; or with nonpredatory price discrimination.

There are several reasons for this shift in antitrust attitudes and enforcement policies.[1] One important reason is the increasing professional disenchantment with the traditional "barriers to entry" doctrine.[2] This theory held that firms in concentrated markets erected economic barriers (such as product differentiation) that unfairly deterred the entry of rivals and allowed dominant firms to exercise "monopoly power." Critics of this theory argued, instead, that most of these alleged economic barriers were simply market efficiencies that served to improve consumer welfare. When and if dominant firms failed to provide such improvements, rivals would inevitably enter markets and compete. Thus, superior economic performance ought not to be attacked prematurely in the name of removing "barriers" to competition.

Another reason for the decline in traditional enforcement policies is the increasing theoretical and empirical criticism of the "concentration doctrine."[3] This theory held that high market concentration encouraged business collusion, and that this was evidenced by higher-than-normal profits (accounting returns) in concentrated markets.

This notion has now been severely criticized. Some analysts have failed to substantiate any long-run empirical relationship between high profit and high concentration.[4] Other analysts have argued that accounting profits are a poor indicator of "monopoly" or resource misallocation,[5] while still others have argued that long-run profits could simply be a return to long-run innovation

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and risk taking.[6] Efficient firms can be expected to earn more than less efficient firms and then grow faster than less-efficient firms.[7] If market concentration is simply the natural consequence of superior economic performance, the role of antitrust regulation (particularly merger policy) becomes ambiguous.

A final reason for the shift in antitrust attitudes is that a reexamination of some of the classic antitrust cases did not support the general rationale for traditional enforcement.[8] In many of the classic antitrust cases, both public and private, the indicted defendant firms had lowered their prices, expanded their outputs, engaged in rapid technological change, and generally behaved in ways consistent with an efficient and rivalrous process. Indeed, it was precisely this rivalrous behavior that may have precipitated the antitrust legal action. There is now a wider recognition among antitrust specialists that competition is a *process*—not an equilibrium condition—and that antitrust (especially in the private cases) may have been employed as a legal club to thwart rivalrous behavior and protect existing market structures.[9]

Yet, despite this impressive theoretical and empirical revisionism, antitrust regulation in the middle of the 1980s is still very much alive and well. None of the antitrust laws have been abolished or even reformed. The Regan administration's modest legislative proposals to modify sections of the Clayton Act (1914) went nowhere with the Congress.[10] Most economists, and even some of the most important antitrust critics, still believe that some antitrust regulation *is* necessary to promote social efficiency and control "market power." Specifically, the Antitrust Division of the Department of Justice and the Federal Trade Commission (FTC) continue to regulate large horizontal mergers, and they still forbid price-fixing and division-of-market agreements, resale-price maintenance, and so-called "predatory practices."

This is not the place to explain the survival of the antitrust paradigm or to challenge the theoretical foundations of all antitrust regulation.[11] Instead, I intend to examine critically the proposition that the current prohibition of predatory practices under the Sherman Act (1890) is rational and consistent with the new theoretical and policy revisionism.

Predatory Practices

Predatory price reductions imply that a leading ("dominant") firm can price its product in ways that may adversely affect smaller rivals, or potential rivals, and reduce market efficiency. A firm, for instance, might temporarily reduce the price of its product below "costs" in an attempt to eliminate a rival or in order to discourage potential entry into some market. Such a price reduction might squeeze a rival's profit margin and might even tend to put a competitor out of business. Alternatively, a dominant firm's price reduction aimed at potential entrants could "discipline" them, and create a "chilling effect"; they might

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decide not to enter the market as a consequence. In antitrust jargon, trade is said to be restrained by such behavior and consumer welfare is allegedly threatened.

A nonprice predatory practice implies that a dominant firm can employ some nonprice rivalrous variable (e.g., product differentiation or advertising) in a way that might raise a rival's costs or reduce the demand for a rival's product.[12] For example, if the dominant firm is a more efficient advertiser, an increase in advertising expenditures that must be matched by a rival could raise a rival's costs. Or some dominant firm might suddenly introduce a revolutionary new industrial process or technology that cannot be duplicated by smaller rivals. The effect of these actions, it is alleged, could be to lessen competition and reduce consumer welfare.

The use of the term *predation* certainly makes such business practices appear antisocial. Those who would defend so-called predatory practices are immediately put on the defensive. Yet, despite the inappropriate jungle terminology, it will be argued shortly that predatory practices in a free market are not inefficient nor are they socially harmful. The regulatory agencies and the courts should not prohibit or attempt to regulate them.

There are several different aspects of predatory behavior that require separate examination. The first concern is the issue of "intent"; the second is whether the dominant firm prices below costs or whether the firm is able to raise, unfairly, a rival's costs; the final concern is the alleged effect that predatory behavior can have on resource allocation and on the welfare of consumers.

Intent

It is sometimes maintained that predatory behavior can be distinguished from normal rivalrous behavior by a careful examination of "intent." The issue becomes: what was the intent (intention) of the dominant seller when it reduced its prices? If the intent of the dominant firm was to eliminate some smaller rival in order to gain "market power," then the practice can be deemed predatory and a violation of antitrust law.

Economists generally have been critical of attempts to distinguish predatory behavior from competitive behavior by focusing on subjective intent. When a dominant firm lowers its prices to hold a declining market share, is that predation or is it just vigorous competition? When a large firm lowers its prices to improve its market share, is that predation or is it competition? When a dominant firm lowers its prices and expands its output in order to discourage market entry by potential suppliers, is that predation or just efficient competitive behavior? Every so-called "explicit" evidence of intent (such as written memos to "get" firm X) is open to various interpretations. Rival suppliers could always assert that the clear intent of the dominant firm's price reductions was predatory, such as the elimination of the smaller rivals (and they could always attempt

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to use the antitrust laws in an attempt to stifle such activity). Dominant firms could always assert that their price reductions, given market uncertainty, were simply part and parcel of a dynamic discovery process. Thus, it should be clear that it is quite impossible to determine whether price reductions are appropriate or inappropriate by focusing on alleged intent.

An even more critical position with respect to the issue of intent is that all pricing, even so-called competitive pricing, does intend to take sales and market share away from rivals. A competitive market process implies that resources tend to shift from less efficient uses to more efficient uses, and this process may mean that some rivals do lose sales and profits. Unlike the atomistic equilibrium condition, rivals in a competitive market process *are* interdependent, and the price reductions of one firm do *aim* to affect the sales of another firm. The intent of a price reduction is to put a company in a better strategic position vis-à-vis rival sellers; the reduction intends to improve the position of a business organization relative to other business organizations. Thus, there is nothing unique about the intent associated with so-called predatory behavior, and there is no realistic way to determine the social correctness of such practices on the basis of intent.

The Debate over Predatory-Pricing Rules

The academic and legal debate concerning the appropriateness of certain price reductions has shifted to whether such reductions are below "cost." Businessmen frequently complain that an aggressive rival is selling at prices that are "below cost" and that such pricing is unfair and even predatory. This "below-cost" charge probably implies that a rival sells at a price that is below its own (short-run) average cost.

Most analysts hold that such short-run pricing behavior is not necessarily predatory. They tend to argue that as long as market price equals at least *marginal* cost, a firm is not engaged in inefficient or predatory pricing. However, if market price were to fall below marginal cost or average variable cost, such pricing might be socially inefficient and might even be predatory.

A voluminous literature (and debate) has developed over price predation and various price–cost rules. Phillip Areeda and Donald Turner originally argued that dominant firm pricing below reasonable anticipated marginal cost (or average variable cost) was predatory and should be illegal.[13] F.M. Scherer responded that the Areeda-Turner rule was both unrealistic and analytically incorrect.[14] He argued that an examination of many other factors was required before any determination could be made that pricing below short-run marginal cost was exclusionary and inefficient. Oliver Williamson noted that the Areeda-Turner rule could still allow dominant firms to operate inefficiently and with "excess capacity."[15] He once recommended that dominant firms be prohibited

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from expanding output for a specified period of time in order to encourage new entry and "competition." The predatory price–cost discussion has continued intermittently in the journals without any definitive theoretical or policy resolution.[16]

A General Criticism of the Debate

There are several ways to criticize the standard form that this predatory pricing debate has taken. The most general criticism is that the debate has assumed that the equilibrium models of pure monopoly and pure competition are appropriate for predatory-price analysis. In this context, the dominant firm (for all practical purposes) is the textbook "monopolist" and its performance is compared (unfavorably) with the firm in a competitive equilibrium. Given this conventional framework, it is not surprising that dynamic pricing (and nonprice) practices on the part of a dominant firm, such as predatory practices, can be construed as resource misallocating.

The assumptions and conclusions of the standard equilibrium analysis are debatable. The textbook monoplist misallocates resources because it is assumed that there are no close substitutes for its product and because it is assumed that there can be no market entry. With the competitive *process* ruled out by definition, it is easy to demonstrate that the dominant seller can charge a price higher than marginal cost. But this allocative inefficiency is totally contrived by the strict *equilibrium* assumptions of the model.

In the real business world, a dominant firm, as distinct from a textbook monopolist, arises and operates under uncertain, disequilibrium conditions. The dominant firm gains and holds its market share by engaging successfully in a competitive market process of discovery and adjustment.[17] And as a consequence of its market-coordinating skills, the dominant firm tends to grow faster than its rivals (or potential rivals) *into* a position (perhaps temporary) of market dominance.

This position of market dominance is not an equilibrium condition and the competitive process has not been extinguished. Changing tastes and technological change still must be discovered and efficiently exploited in order to maintain (or increase) market position. Economic profits will still act as an incentive for additional output, and industry resources will still tend to flow from less profitable uses to more profitable uses. Unlike the textbook monopolist, the competitive process will continue to "swirl around" the dominant firm, continuously creating incentives for efficient plan coordination.[18] Price will tend toward "cost" (everything else equal) and profit incentives will drive cost curves to a minimum level. Thus, it is unreasonable to assume that the dominant firm can behave like an equilibrium "monopolist" or that it can inherently misallocate resources.

If the preceding is correct, the standard approach errs when it attempts to compare the performance of the dominant firm negatively with the performance of

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firms in competition. Resources are allocated efficiently in the competitive equilibrium only because firms, in the absence of uncertainty, must charge prices equal to marginal cost. The dominant firm, on the other hand, may charge prices that are higher (or lower) than marginal cost. The standard welfare conclusion is that competitive firms are more efficient than dominant firms.

But this conclusion is fully contrived by the equilibrium assumptions of the analysis. It is assumed that the competitive process has completely *ended* in competition (that is, that price and marginal cost are equal). With tastes and technology assumed constant and market information assumed perfect, all of the important sources of market inefficiency have simply been assumed away. Prices already equal marginal cost in the competitive equilibrium; any real-world comparison with this equilibrium condition *must* appear unfavorable.[19] Prices only *tend toward* cost for the dominant firm in disequilbrium. But of what possible *policy* significance is such an approach?

> The economic problem to be solved by competition is emphatically *not* one of how resources would be allocated if information were perfect and consumer tastes constant; with everything known and constant, the solution to such a resource-allocation problem would be trivial. Rather, the economic problem lies in understanding how the competitive market process of discovery and adjustment works to coordinate anticipated demand with supply in a world of imperfect information. To assume away divergent expectations and change, therefore, is to assume away all the real problems associated with competition and the resource allocation process.[20]

The irrelevance of the equilibrium paradigm for judging the efficiency and appropriateness of certain dynamic business practices (such as predatory pricing) can now be made explicit. Nothing that is truly rivalrous can occur in the static competitive equilibrium; after all, the coordinating process has been completed in equilibrium. All of the business practices that we associate with rivalrous behavior occur in a competitive disequilibrium, not in "competition." For example, product differentiation and advertising have been seen as resource misallocating but *only* within a competitive equilibrium framework. Once competition is understood as a process of discovery and adjustment under conditions of uncertainty—and not as a static equilibrium condition—product differentiation, advertising, and all price reductions can easily be reconciled with increasing market coordination and with increasing market efficiency.

A Criticism of the Price–Cost Rules

With respect to any specific price–cost rule, it can be admitted readily that business "costs" are very difficult to define and even harder to accurately measure, especially long-run marginal costs. More importantly, however, all cost-based pricing rules for "efficient" pricing are inherently suspicious. Real-world market

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prices are determined by utility and revealed preferences and not directly by accounting costs. Accounting costs may help determine market supply in some previous time period, but current demand conditions—given market supply—actually determine transactions prices. And they determine transactions prices in each successive time period.

Another fundamental objection to any cost-based pricing rule (for determining predatory practices) is that the actual costs which do affect decision making are not accounting costs at all, but are subjective opportunity costs.[21] Opportunity costs are personal and subjective and they are known only to the decisionmaker and then only at the moment of decision. Thus, whether actual market prices are above or below specific historical accounting costs may be relatively insignificant, since the accounting costs themselves may not be the relevant costs for decision making.

Analysts are often misled on this issue because of their predilection for pricing in the competitive equilibrium. *If* the business world were purely competitive, costs could (in some sense) be said to determine market prices. But the actual business world is not, and cannot, be purely competitive, nor can it be in equilibrium. The existence of product differentiation, transactions costs, changing information, and uncertainty—both short- and long-run—all prevent the realization of any static competitive equilibria.[22] And in a world of uncertainty and change, all cost-determining-price rules, based on hypothetical conditions in some static equilibrium, become irrelevant for policy purposes. Yet most of the criticisms of firm predation and of business "excess capacity" are deeply rooted in static equilibrium welfare analysis.

It is important to understand that a competitive market process does create powerful incentives for entrepreneurs to allocate resources such that market price and factor costs do *tend* toward equality, other things remaining the same. But other things (that is, market information and tastes) cannot actually remain constant. And since the market process can never be completed, the static long-run equilibrium condition can never be actually realized. (It would not be "ideal" even if it were completed.) Thus, it is a serious policy mistake to regard any divergence of price from explicit cost (average or marginal, short-run or long-run) as evidence of social inefficiency or of predatory pricing. It may only be an indication of a competitive market process at work under inevitable disequilibrium conditions.

The Shepherd Proposal

William G. Shepherd has argued that all "intent" and price-cost rules are unsatisfactory, and that the only two variables that should be relevant in determining illegal predation are market share "disparity" and "selective" action.[23] Practices are predatory and unfair if the "attacking" firm is "dominant" (in terms of its market share relative to the firm being "attacked") and if the firm employs

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"selective actions that are unavailable to its competitors." To allow such practices, according to Shepherd, would be unwise since it would make market competition "increasingly one-sided and ineffective."[24]

This is a curious argument for a public policy designed, presumably, to promote efficiency and consumer welfare. Dominant firms—that is, successful firms—are not to be allowed to initiate selective price or nonprice policies unless such practices are available (at the same cost?) to rivals. The matter can be put another way. Consumers of products are not to be provided with selective advantages by successful firms unless these advantages can be provided (at the same cost) by (all?) smaller rivals. Dominant firms are not to innovate selectively unless all firms can innovate. Dominant firms are not to advertise unless everyone can advertise at the same cost. Dominant firms should not provide special ("selective") services to specific customers unless smaller rivals are able to employ "comparable competitive actions." One could hardly imagine a predatory legal "rule" more destructive to the competitive market process than the one envisioned by Shepherd (or Williamson).

Dominant firms have achieved their position of "dominance" in free markets by being successful. They have innovated the products and services that consumers prefer relative to rivals and potential rivals. That is why, presumably, these firms have grown faster than rivals and have become dominant. To specifically inhibit the rivalry of such business organizations *after* they have demonstrated their market efficiency does not appear consistent with a genuine concern for either efficiency or consumer welfare.

Shepherd and others appear to have fallen into a familiar theoretical antitrust trap: they have equated increased "competition" with an increasing number of business organizations or with a tendency toward more "equal" market shares. Policies that promote increasing numbers of entrants or that lower the market share of the dominant firm are simply accepted as "good." Policies that tend to eliminate less efficient suppliers and restrict the entry of high-cost entrants are seen as "bad." Yet, it should be obvious that the most appropriate policy from this perspective—but the worst policy for consumers—would be one where a dominant firm reduced its outputs, raised its prices, and refused to innovate. Such a policy would severely punish consumers, but it would not "threaten" any smaller rival; no smaller competitor would ever feel that it was under attack from the dominant firm. In fact, the more inefficient the dominant firm became, the better it would be from this perspective. Even government tariff protection would "help" since it would tend to foster a more "comparable competition." It is difficult to see how any of this is compatible with a genuine concern for consumer welfare.

Raising Rivals' Costs

A currently fashionable theory of business predation holds that a dominant firm can unfairly raise a rival's costs and thereby lessen competition in the marketplace.

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Economists Salop and Scheffman have suggested that certain business practices such as boycotts, exclusive dealing, research and development spending, and even advertising can be employed by a dominant firm to increase a rival's costs. For example, if "advertising expenditures initiated by the most efficient advertiser must be matched . . . by less efficient rivals,"[25] there could be a potentially predatory problem that might require an antitrust remedy.

This is a very dangerous line of "reasoning." It is distinctly reminiscent of an earlier antitrust era where every economic advantage or technical efficiency possessed by any firm was seen, incorrectly, as a pernicious barrier to entry. Annual autobody-style changes (product differentiation) on the part of the dominant auto companies made it difficult for small firms to compete because it unfairly raised the costs of competition.[26] Economies of scale in production, transportation, and finance, or absolute economies associated with some revolutionary technology, might limit the entry of higher-cost suppliers. The FTC argued that successful advertising by Kellogg's and the other "dominant" ready-to-eat cereal manufacturers could make it difficult for smaller cereal suppliers to gain and hold market share.[27]

The basic error in this approach is that the overall purpose of the competitive process is forgotten. The competitive process is necessary in order to discover *what* consumers prefer and to discover *which* business organizations can supply those goods.[28] Consumer-approved product differentiation may well make it more costly for newer business organizations to compete, but this does not mean that the result is socially inefficient or requires any antitrust remedy.

Efficiency implies that resources should be put to uses that consumers value most highly. If consumers support annual auto-style changes, then *that* is the use to which resources should be put. Potential suppliers or existing smaller rivals can always attempt to convince consumers to support less product differentiation (or advertising)—at a lower price—or perhaps no year-to-year differentiation at all. Alternatively, potential entrants can always attempt to discover cheaper methods of production (which is what the Japanese auto companies did in the 1970s) that would allow increased rivalry with dominant firms. But, in the absence of such preference changes or discoveries, potential competitors are only "restricted" from additional production or higher market shares by the superior overall performance of the dominant companies and the revealed preferences of buyers. Performance and preference are entirely appropriate "restraints" on the entry of would-be business organizations. To describe and condemn such "barriers" as exclusionary or as predatory practices seriously misconstrues the social purpose of the market process.

Predation and Consumer Welfare

The literature on predation emphasizes that it is the predatory practices of the dominant firm that can eliminate rivals and lower consumer welfare. Yet,

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predatory practices—both price and nonprice—cannot succeed at all without direct consumer/buyer support.[29] For example, if a dominant firm reduces its prices and prospective buyers choose to ignore these price reductions, then the price reduction cannot really be predatory. Potential consumer/buyers can decide, for instance, to preserve the number of rival suppliers by ignoring the price reduction and by continuing to purchase in the same old patterns. On the other hand, if consumer/buyers *do* alter their preferences and decide to support the price cutter, it is the buyers—and not the price cutter—that put pressure on the high-price firms, and it is the buyers—not the price cutter—that may ultimately eliminate some of the rival suppliers. But consumer/buyers can *always* eliminate certain suppliers by altering their buying preferences and choosing one product (for whatever reason) over another. Why should consumer/buyers be prevented by antitrust law from reallocating industry resources from high-price sellers to low-price sellers? How is this legal restraint in the consumers' interest?

It will not suffice to argue that such choices (to reward the price cutter) are not really in the long-run interests of buyers. No one can know (in advance) the long-run interests of buyers. Further, why are so-called long-run interests superior to short-run interests? Buyers can surely decide their *own* time preferences and then decide whether the advantages of short-run price reductions exceed the probable future disadvantages of fewer suppliers. Consumer choices are rational either way, and consumer "welfare" is only reduced when antitrust policy prevents consumers from determining the market supply structure that they apparently *do* prefer.

The same argument holds true with respect to nonprice predatory practices. Indeed, the relevant issues are exactly the same. If a dominant firm suddenly introduces some new innovation, it is up to consumers to decide whether that innovation ought to reduce the number of rivals or not. If they enthusiastically support the innovation at the expense of some rival products, then such decisions by consumers may well tend to eliminate specific suppliers. On the other hand, if consumers do *not* support the innovation, the innovation cannot threaten "competition" and cannot be predatory. In neither scenario is there a legitimate rationale for antitrust regulatory preferences to supersede the revealed preferences of buyers with respect to the pace and nature of technological change. Indeed, it would be difficult to imagine an intervention on the part of the antitrust authorities or the courts that would be as potentially dangerous or damaging to future consumer welfare as this sort of innovation regulation.[30]

Market Structure and the Competitive Process

One implication of this discussion is that buyers in a free market may (occasionally) decide to support only one (or a few) major business organizations in

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some specific line of commerce. Such an extreme supplier situation is certainly possible (although not typical) and there is no reason, from a market-process perspective, to object to *any* momentary market structure of suppliers. After all, free markets are always open to potential rivalry and entry, and dominant suppliers can attempt to maintain their market positions only by maintaining an overall efficiency advantage over potential users of resources. This efficiency advantage can be produced either by a process of rivalry or by a process of interfirm cooperation. If dominant firms were to attempt to exercise any so-called monopoly power by reducing market output and raising market price, this behavior would tend to negate the market advantages that allowed such organizations to gain their dominant market positions in the first place. Such practices would tend to result in a severe loss of market share to alternative suppliers and, thus, create strong disincentive to attempt such practices in the first place. Government or legal restrictions on market entry might create some incentives to exercise monopoly power. But legally open markets would create continual incentives for dominant firms to be relatively more efficient than their rivals or potential rivals. Thus, *any* market structure of suppliers is compatible with an efficient market process as long as there are no legal barriers to entry.

Predatory Practices in Reality

John McGee has long been associated with the idea that predatory practices are not generally rational or efficient ways of gaining (or holding) a dominant market share.[31] Firms that engage in predatory pricing (pricing below cost) would stand to lose a considerable amount of revenue or profit funding any predatory "war." If the dominant firm is large, the opportunity costs (lost profit) and risks (the war spreads; the length is indeterminate) are sure to create substantial disincentives to engage in such activity. In addition, target rival firms may not easily be driven from business or, even if they are, their assets may be acquired by new business organizations that are willing to compete again as soon as the predatory price is lifted. In short, there are considerable financial risks associated with long-term price predation, and such risks create powerful disincentives for such practices, especially in industries with no legal barriers to entry.

Some laboratory and case-study evidence would appear to confirm the theoretical speculations concerning the disincentives associated with severe predatory price practices.[32] There are few unambiguous examples in business history where leading firms have attempted to gain or hold dominant market positions by engaging in extensive predatory practices.[33] Even the allegedly classic examples of such practices in the nineteenth-century petroleum and tobacco industries (involving Standard Oil and American Tobacco) are either exaggerated or unfounded. Standard Oil secured its market position in petroleum

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primarily through internal efficiency and merger and not through systematic predatory practices.[34] And while the American Tobacco Company may have occasionally employed severe price competition to gain market share—the great "snuff war" comes to mind—no generally predatory policy would have been intelligent (that is, profitable) in an industry (tobacco) with thousands of competitive suppliers and with no barriers to market entry.[35] Even when severe price competition did occur in the tobacco industry, consumers enjoyed these price "wars" immensely by purchasing greatly expanded volumes of tobacco products at very low prices—for years. Why antitrust should attempt to restrain such occasional practices—practices that so clearly benefit consumers directly—is not obvious.[36]

Conclusion

This article has suggested that the general theory of predatory practices is seriously flawed. Predatory behavior cannot be logically distinguished from benign competitive behavior either by intent or by any price–cost rules. Price reductions, selective or otherwise, and various nonprice rivalrous strategies (such as advertising and innovation) are all part and parcel of a competitive market process. This process serves an important social purpose: it serves to discover the products and services that consumers prefer, and the business organizations that can provide those products and services. Antitrust regulation of this process is based on inappropriate equilibrium theorizing, and it serves only to inhibit the discovery of consumer preferences and the flow of resources from less efficient suppliers to more efficient suppliers.[37] Thus, the legal restriction or prohibition of any competitive practice is inappropriate and appears contrary to the newer antitrust reform attitudes.

Notes

1. James Langefeld and David Scheffman, "Evolution or Revolution: What Is the Future of Antitrust?" *Antitrust Bulletin* (summer 1986): 287–99.

2. Robert H. Bork, *The Antitrust Paradox: A Policy at War with Itself* (New York: Basic Books, 1978), chapter 16. See also Harold Demsetz, "Barriers to Entry," *American Economic Review,* 72 (March 1982): 47–57.

3. Harold Demsetz, *The Market Concentration Doctrine,* (Dallas, Tx.: American Enterprise Institute—Hoover Institution Policy Studies, August 1973).

4. Yale Brozen, "Concentration and Profits: Does Concentration Matter?" *Antitrust Bulletin,* 19 (1974): 381–99.

5. Franklin M. Fisher and John L. McGowan, "On the Misuse of Accounting Rates of Return to Infer Monopoly Profits," *American Economic Review,* 73 (March 1983): 82–97.

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6. S.C. Littlechild, "Misleading Calculations of the Social Costs of Monopoly Power," *Economic Journal,* 91 (June 1981): 348–65.

7. Harold Demsetz, "Industry Structure, Market Rivalry, and Public Policy," *Journal of Law and Economics,* 16 (April 1973): 1–10. See also John R. Carter, "Collusion, Efficiency, and Antitrust," *Journal of Law and Economics,* 21 (October 1978): 434–44.

8. Dominick T. Armentano, *Antitrust and Monopoly: Anatomy of a Policy Failure* (New York: John Wiley & Sons, 1982).

9. William Baumol and Janusz Ordover, "Use of Antitrust to Subvert Competition," *Journal of Law and Economics,* 28 (1985): 247–65.

10. For a discussion of the Reagan antitrust proposal see *Washington Post,* January 19, 1986, pp. H1, 4, and *Washington Post,* January 20, 1986, p. E1. Also see *The Economist,* August 23, 1986, p. 25.

11. D.T. Armentano, *Antitrust Policy: The Case for Repeal* (Washington, D.C.: Cato Institute, 1986).

12. Steven Salop and David T. Scheffman, "Raising Rivals' Costs," *American Economic Review,* 73 (May 1983): 267–71.

13. Phillip Areeda and Donald Turner, "Predatory Pricing and Related Practices under Section 2 of the Sherman Act," *Harvard Law Review,* 88 (February 1975): 697–733.

14. F.M. Scherer, "Predatory Pricing and the Sherman Act: A Comment," *Harvard Law Review,* 89 (1976): 869–90.

15. Oliver E. Williamson, "Predatory Practices: A Strategic and Welfare Analysis," *Yale Law Journal,* 87 (December 1977): 284–340.

16. See, for example, Paul L. Joskow and Alvin K. Klevorick, "A Framework for Analyzing Predatory Pricing Policy," *Yale Law Journal,* 89 (December 1979): 213–70. This article is offered as a "decision-theoretic framework . . . for evaluating alternative predatory pricing rules" (p. 270). The courts have paid some attention to this debate. See *International Air Industries and Vebco, Inc.* v. *American Excelsior Company,* 517 F.2d 714 (1975); *O. Hommel Co.* v. *Ferro Corp.* 472 F. Supp. 793 (1979); and especially *Northeastern Telephone Company* v. *American Telephone and Telegraph et al.,* 605 F.2d 201 (1981).

17. Israel Kirzner, *Competition and Entrepreneurship* (Chicago: University of Chicago Press, 1973).

18. Joseph Schumpeter, *Capitalism, Socialism and Democracy* (New York: Harper Torchbooks, 1950).

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22. Gerald P. O'Driscoll, Jr., and Mario J. Rizzo, *The Economics of Time and Ignorance* (Oxford, England: Basil Blackwell, 1985).

23. William G. Shepherd, *The Economics of Industrial Organization,* 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1985), pp. 270–74.

24. Ibid., p. 274.

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25. "Raising Rivals' Costs," *American Economic Review,* 73 (May 1983): 267–71.

26. Franklin Fisher, Zvi Griliches, and Carl Kaysen, "The Costs of Automobile Model Changes since 1949," *Journal of Political Economy,* 70 (October 1962): 433–51.

27. *FTC* v. *Kellogg, General Mills, General Foods, and Quaker Oats,* Docket No. 8883 (1972). (The complaint was dismissed in 1982.) See also Richard Schmalensee, "Entry-Deterrence in the Ready-to-Eat Breakfast Cereal Industry," *Bell Journal of Economics,* 9 (1978): 305–27.

28. Israel Kirzner, *Competition and Entrepreneurship* (Chicago: University of Chicago Press, 1973).

29. *Antitrust Policy: The Case for Repeal,* pp. 42–44.

30. Betty Bock, *The Innovator as an Antitrust Target,* Conference Board Information Bulletin, no. 174 (1980).

31. John S. McGee, "Predatory Price Cutting: The Standard Oil (N.J.) Case," *Journal of Law and Economics,* 1 (October 1958): 137–69; and "Predatory Pricing Revisited," *Journal of Law and Economics* 23 (1980): 289–330.

32. R. Mark Isaac and Vernon L. Smith failed to discover any price predation in a laboratory experiment. Moreover, simulating antitrust remedies for predation (such as the Williamson output restriction) had the unintended effect of raising prices and reducing efficiency. See R. Mark Isaac and Vernon L. Smith, "In Search of Predatory Pricing," *Journal of Political Economy,* 93 (April 1985): 320–45.

33. Ronald H. Koller, "The Myth of Predatory Pricing: An Empirical Study," *Antitrust Law and Economics Review,* 4 (summer 1971): 105–23. See also Kenneth G. Elzinga, "Predatory Pricing: The Case of the Gunpowder Trust," *Journal of Law and Economics,* 13 (April 1970): 223–40.

34. *Antitrust and Monopoly,* pp. 55–73.

35. Ibid., pp. 85–95.

36. Frank Easterbrook concludes, after a seminal criticism of the predation literature, that such practices (should they occur) generally enhance consumer welfare and should be *per se* legal. See Easterbrook, "Predatory Strategies and Counterstrategies," *University of Chicago Law Review,* 48 (spring 1981): 263–337.

37. There are many (mostly private) legal cases that substantiate this position. See, for instance, the classic *Utah Pie Co.* v. *Continental Baking Co., et al.,* 386 U.S. 685 (1967). See also *Berkey Photo, Inc.,* v. *Eastman Kodak Company* (457 F. Supp. 404 [1978] and 603 F.2d 263 [1979]) where the plaintiff (Berkey) argued that the successful innovation of Kodak's new instamatic camera (and film) was "predatory" and hindered its ability to compete. The thrust of most of the (private) predatory legal cases is an attempt by plaintiffs to restrain the competitive rivalry of the more successful defendant firms. In short, antitrust law has been employed in an attempt to restrain trade.

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Why the U.S. Economy Is Not Depression-Proof

Mark Skousen

> If the monetary policies of the 1920s brought forth the Great Depression, similar policies during the 1980s are likely to produce another depression.
>
> —Hans F. Sennholz[1]

In 1954, Milton Friedman delivered a lecture in Stockholm, Sweden, entitled, "Why the American Economy Is Depression-Proof."[2] In many ways, his published speech symbolized the new bold optimism of the contemporary economists in a post-Keynesian world. Economists and government officials, according to Friedman, have sufficient understanding of the interworkings of the whole economy and the technical tools with which to prevent an economic downturn from turning into a full-scale depression. While considered a maverick on most subjects, on this issue the illustrious Chicago economist joined the chorus of neoclassical orthodoxy in unanimously proclaiming that another 1930s-style debacle is impossible.[3]

Friedman referred to several institutional changes made by government since the 1930s that would "render a major depression in the United States almost inconceivable at the present time."[4] These fundamental developments included the establishment of federal insurance on bank and savings deposits, the abandonment of the gold standard, and the substantial increase in the size of government and the welfare state.

The demonetization of gold was a critical step in the Federal Reserve's ability to ward off a major slump. Friedman cogently argued that defending the gold standard during a period of credit expansion would eventually force a monetary collapse, as it did in 1929–33. The removal of any barriers to monetary inflation is essential, he said, since "there has been no major depression that has

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not been associated with and accompanied by a monetary collapse."[5] In short, Friedman believes that a depression can be avoided as long as the money supply does not decline.

Friedman's lecture was given at a time when there was considerable concern that a mild recession in 1953 would degenerate into a major depression. He cited Colin Clark, a prominent British economist, as one who held this pessimistic view. But the Chicago monetarist denied such a possibility, stating confidently that "anything more than a minor economic recession is extremely unlikely."[6] Over the longer term, he forecast "a period of recurrent bouts of inflation produced by overreaction to the temporary recessions that punctuate the period."[7] He also predicted that the inflation would not turn into a runaway inflation.

Friedman's lecture has proven remarkably prophetic so far. The 1953 recession ended officially in mid-1954. Since then, the United States has experienced a series of economic expansions, punctuated by occasional contractions, but none severe enough to qualify as a 1930s-style depression. Throughout the past thirty-five years, the economy has faced a general rise in consumer prices, but no runaway inflation.

Have Friedman's Views Changed in Thirty-five Years?

What about today? In the face of the stock market panic in October 1987 and renewed predictions of either depression or runaway inflation, does Friedman see things differently? Apparently not. Referring to his 1954 lecture, he recently wrote: "I have seen no reason since then, and see none now, to change that conclusion."[8] Furthermore, he states elsewhere:

> I do not expect any repeat of the Great Depression. I expect another garden-variety type of recession unless you have strongly protectionist trade legislation come out of the Congress plus undesirable tax increases. In that case, the betting is off and the recession might be much more severe than I now anticipate.[9]

Why the American Economy Is Now Vulnerable

My thesis is that Friedman's "built-in stabilizers" are not a sufficient condition to prevent the U.S. economy from suffering a devastating economic debacle some time in the future.[10] For several reasons to be outlined shortly, I believe that the U.S. economy suffers from certain structural defects that under the right circumstances could precipitate a financial disaster similar in scope to the 1929–32 crisis.

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I am well aware of the fact that numerous free-market economists and hard-money investment advisors have predicted economic calamity over the past two decades.[11] So far their dire forecasts have not materialized because they underestimated the government's ability to defuse the crises and postpone deflation. But now I believe we are entering a new era that could be more dangerous than the 1970s or 1980s. Although a future economic crisis may not produce the degree of unemployment and other marked effects associated with the Great Depression in the 1930s, it could involve a substantial reduction in the standard of living of most Americans for a period of time.

The Definition of a Depression

Before presenting my arguments, I need to make clear what is meant by a depression. The same question was asked of Friedman following his lecture in Sweden. Friedman adopted the traditional view of defining a depression in terms of the level of unemployment. Although admitting that the distinction between a recession and depression is statistically imprecise, Friedman said, in essence, that he would consider an 8 percent unemployment rate to be a "mild recession," 8 to 13 percent to be a "severe recession," and 14 to 25 percent or more to be a "depression."[12]

The economic emergency I am expecting could conceivably cause the rate of unemployment to reach 15 percent or more, but the definition of depression should include other measurements in addition to the level of unemployment. The definition of depression should be expanded because, in an age where the government views itself as an employer of last resort, the country could face a severe depression while official unemployment statistics may remain artificially low due to ubiquitous government hiring.

In many socialist countries, the government is the principal employer. Consequently, officially there is little or no unemployment, even though citizens are undoubtedly employed in an inefficient manner (commonly referred to as "underemployment"). It is quite conceivable that an economic crisis could be of such magnitude in the United States that the federal government would attempt to employ millions of Americans, in a civilian or military status, in an effort to keep official unemployment statistics politically acceptable. Such a makeshift solution might be a way of spreading the misery around, but it would not eliminate the misery and would, in fact, increase it by reducing the incentive for productive citizens to work.

A depression should be properly defined as a substantial decline in the standard of living. A common way to determine material well-being is to measure the year-to-year change in individual income levels, adjusted to account for changing purchasing power of the national currency. It is imperative that nominal incomes be adjusted by price changes. In the case of a deflation, price reductions would enhance nominal income. In the case of an inflation, especially

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a runaway inflation, price increases would be detrimental. History has shown that depression—that is, substantially lower standards of living—is possible in times of either rising or falling prices. While price figures may not be accurate, especially if they are manipulated by the government data gatherers, they can reflect the general decline in people's material well-being. Therefore, as a rule of thumb, I would define a depression as a period of time (say, one to five years) when average real incomes decline substantially (say, 30 percent or more). This decline in real income would undoubtedly coincide with significant unemployment and underemployment of labor and resources. In short, the United States and other western countries could suffer from serious macroeconomic disequilibrium.

The use of per capita real income may not completely capture the depth of an economic downturn, however. It does not take into account, for example, the number of family members that may be forced by economic necessity to seek employment. The increasing number of women in the work force in the 1970s and 1980s was not simply a response to the women's liberation movement, but reflected the increased necessity of earning a higher family income in order to maintain the same standard of living in an inflationary environment. Furthermore, if the government placed large numbers of the unemployed on its payroll, per capita income figures might not reflect the sharp decline in the standard of living.

Worse than 1929?

In order for a future depression to be "worse than 1929," as Hans Sennholz predicts, we would need to see:

1. Gross national production (in real terms) decline by more than 30 percent,
2. Per capita personal income (in real terms) drop by 28 percent or more,
3. Private investment (in real terms) fall by more than 86 percent,
4. Stock prices plunge by more than 80 percent,
5. The unemployment rate climb by over 25 percent,
6. Retail prices drop by an average of 24 percent, wholesale prices by 31 percent, and raw commodity prices by 42 percent,
7. The business bankruptcy rate rise by 50 percent or more, and
8. Nearly half the commercial banks fail.[13]

The magnitude of the Great Depression is overwhelming; it is hard to conceive of it happening again. The two worst recessions the United States has experienced since the 1930s occurred in 1973–75 and 1980–82. If we examine

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Table 1
Recent Recessions versus the Great Depression: Selective Statistics

Factor	*1929–32*	*1973–75*	*1980–82*
Real GNP	−30%	−3%	−1%
Per capita real income	−28%	−1.1%	0.0%
Private gross investment (real)	−86%	−31%	−22%
Unemployment	25%	8%	11%
CPI	−24%	+26%	+30%
Stock prices	−88%	−45%	−25%
Money supply (M1)	−21%	+9%	+16%
Government expenditures as a percent of GNP	10%	35%	40%

Source: *Statistical Abstract of the United States* (Washington, D.C.: U.S. Department of Commerce, 1980, 1987); *Historical Statistics of the United States: Colonial Times to 1970,* (Washington, D.C.: U.S. Department of Commerce, 1975); and *Business Conditions Digest* (Washington, D.C.: U.S. Department of Commerce, 1987).

Note: Stock prices based on New York Stock Exchange Composite Index.

in table 1 a few selective statistics, we see that the 1973–75 and 1981–82 recessions pale by comparison.

Undoubtedly the substantial increase in the size of government played a significant role in preventing the GNP from declining much during the recessions in 1973–75 and 1980–82. On the other hand, the large size of government did not prevent private investment from falling sharply and unemployment from rising significantly. Another important observation is that the money supply, as measured by M1 or M2, did not decline in absolute terms during 1973–75 and 1980–82. Nevertheless, the economy suffered two severe recessions. Despite Friedman's contention that a depression is impossible without a contraction in the money supply, it is clear that a severe economic recession is conceivable even while the central bank continues to inflate.

Still the question remains: what catastrophic event could precipitate a depression equal or greater in magnitude than 1929–32?

Instability and the Banking System

I begin my case with a central point on which Friedman and I agree: whether or not we have another depression depends primarily on the banking system. The banking system is the linchpin of financial and economic stability in the world. The only way the economy could collapse (other than by war or acts of God) is by the public losing faith in the monetary system of this country. I do not accept the popular conservative view that an excessive national debt could alone cause a depression.

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Figure 1. U.S. Bank Failures, 1934–85

Source: VERIBANC, Inc. (Woburn, Mass. 01888) 1988; from Federal Deposit Insurance Corporation Statistics. Reprinted with permission.

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The recession turned into a depression primarily in the 1930s because of bank failures, which in turn caused the money supply to decline dramatically. (However, the length of the 1930s depression was inordinately long due in part to the inflexibility of wages and other forms of government intervention.) Because of the extremely low level of cash reserves held by the commercial banks in the early 1930s, the demands for cash by nervous depositors resulted in a nationwide financial panic; one bank failure led to another, and in the end, there was a massive contraction in the monetary aggregates.[14]

Friedman maintains that the establishment of federal deposit insurance has virtually eliminated banking panics and bank failures. "In my view, the federal insurance of deposits is by all odds the most important of these changes in its effects on the cyclical characteristics of the American economy." More to the point, "Federal deposit insurance has made bank failures almost a thing of the past."[15]

While his statement about bank failures was accurate in 1954 and in 1968, when he updated the article for *Dollars and Deficits*, it is no longer true, especially since the early 1980s. Figure 1 shows that the number of U.S. bank failures is growing rapidly.

There is little evidence that this alarming trend in abating. According to Veribanc, Inc. (a private independent rating service of U.S. financial institutions), the banking industry in general has been steadily deteriorating since the early 1980s, based on a variety of indicators. Using data provided by the Federal Deposit Insurance Corporation (FDIC) and other bank regulatory agencies, the number of banks operating at a loss is substantial. In the fourth quarter of 1987, 3, 554 banks (or 26 percent of all commercial banks) were losing money. (This is down slightly from the fourth quarter of 1986, but banking experts considered it an anomaly.) During 1985–87, the number of banks classified in Veribanc's "red" category, signifying those banks that will be forced into liquidation if losses continue at the same rate, increased from 514 to 635. The number of commercial banks declined slightly, from 14,344 to 13,616, even though the economy was in the midst of the Reagan boom.

The financial condition of the savings and loan industry is much worse. The number of S&Ls operating at a loss increased from 679 in the fourth quarter of 1985 to 1,068 in the fourth quarter of 1987. Nearly one-third of all S&Ls have a tangible net worth below zero.

Figure 2 demonstrates the secular trend in the banking industry in terms of the number of federally insured commercial banks that could reach zero equity in less than twelve months.

Is Federal Insurance Destabilizing in the Long Run?

What can explain the secular deterioration in the banking industry? Perhaps one of the reasons long-term instability has been the fact that banking

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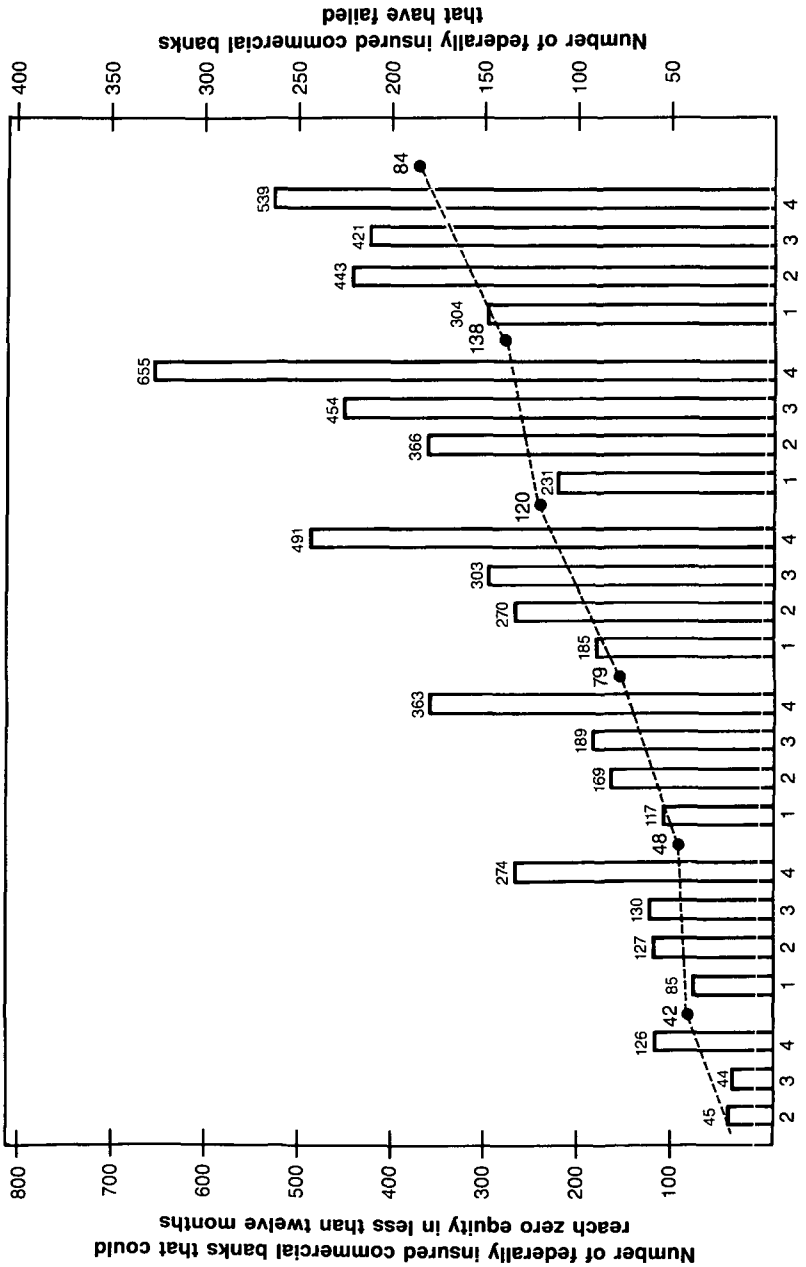

Figure 2. Deterioration of the Banking Industry, 1982–87.

Source: VERIBANC, Inc. (Woburn, Mass. 01888) 1988; from Federal Reserve Statistics. Reprinted with permission.

deregulation has been a gradual process, in contrast to the airline industry, which was quickly deregulated in the early 1980s and appears to have achieved relative stability by the late 1980s.

Part of the responsibility may also rest with federal insurance itself. Under current law, checking and savings deposits are guaranteed up to $100,000 per account by a federal agency. By making customers' deposits virtually risk-free investments, the government is indirectly encouraging bank managers to take greater speculative risks. The theory is that financial officers will take greater chances with depositors' funds if depositors are unconcerned about the quality of the bank's portfolio. This appears to have been the case in the 1970s and early 1980s, when major banks across the country invested in high-risk deals in oil, real estate, and Third World obligations. In the long run, all forms of government intervention backfire.

Moreover, a majority of investors ignore the safety of banks and simply seek out the highest yields on CDs, encouraging the growing problem, especially in the S&L industry, of paying above market yields on savings deposits in a desperate attempt to avoid default.

As it now stands, it will require substantial injections of new funds to cover the insured liabilities of customers' funds in banks and savings institutions that will fail over the next few years. The FDIC currently has reserve assets worth only 1.2 percent of the $1.75 trillion of insured commercial-bank deposits. Of the FDIC's $20 billion in reserves, a large portion is comprised of yet-to-be-sold assets of failed banks. Veribanc estimates that liquid funds amount to between $4 billion and $10 billion, representing a mere 0.5 cents of available FDIC reserves per dollar of insured deposits.

The Federal Savings and Loan Insurance Corporation (FSLIC) publicly acknowledged its insolvency in 1987. Congress issued $10.8 billion in bonds to recapitalize the FSLIC. However, recent closures and consignment actions against several large thrifts have already drawn on a significant portion of this amount. The 339 S&Ls that were insolvent by regulatory standards at the end of 1987 were continuing to lose money at the rate of $9 billion a year, with the regulatory net worth of these institutions amounting to a negative $14 billion. Veribanc estimates that the total tangible net worth of all savings institutions was minus $54.4 billion in 1987. It believes that Congress will be required to inject between $25 billion and $50 billion into the FSLIC over the next few years to pay all the liabilities of the defaulted banks.

How Fragile Is the Financial System?

Because the commercial banking and savings structure is still built on a fragile fractional reserve system, a widespread distrust of the banks by the general public could cause a massive hemorrhaging of the financial system. If the United

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States adopted a 100 percent reserve system, as Friedman and other monetarists have advocated in the past, the financial system would be on a much sounder basis and would make a credit collapse highly improbable.[16]

Unfortunately, however, the United States has not removed this point of instability. Hence, the banking system is just as vulnerable as it was in the 1930s, in terms of low cash reserves in relation to demand deposits. Under the Depository Institutions Deregulation and Monetary Control Act of 1980, most financial institutions are required to maintain only 12 percent of their checking-account deposits in the form of cash (bank notes) or non–interest-bearing reserves at the Fed. (See table 2 for reserve requirements.)

The government has demonstrated its ability to control the financial system in the face of large bank failures, as in the case of the Continental Illinois Bank in 1984. As long as only a few banks fail, occasionally, the government should be able to contain the ongoing crisis and keep it from spreading throughout the country. While the number of annual bank failures has risen sharply since 1980, it has not yet approached the level of the 1930s depression, when nearly half the commercial banks closed their doors permanently. However, if numerous financial institutions (including several large banks) begin closing their doors, there could be a universal effort by customers to convert deposits into cash. The result would be a massive liquidity squeeze. Theoretically, only 12 percent of checking-account customers would be able to obtain cash, although, in reality, the figure is significantly lower since most reserves are actually held in a noncash form at the Fed. The percentage is even smaller for holders of savings accounts and time deposits because of lower reserve requirements. Commercial banks in fact have only approximately $24.5 billion in actual cash (coins and currency) on hand, according to the Veribanc. This represents only a minuscule 4.5 percent of checking-account deposits, and 1.3 percent of all savings deposits! Depending on the severity of the crisis, the

Table 2
Legal Reserve Requirements under 1980 Banking Act

Type of Deposit	*Reserve Requirement*	*Range in Which Fed Can Vary*
Checking accounts		
First $30 million	3%	no change allowed
Above $30 million	12%	8–14%
Time and savings deposits		
Personal	no required reserves	
Nonpersonal		
Up to 1½ years maturity	3%	0–9%
Maturity of over 1½ years		0–9%

Source: Paul A. Samuelson and William D. Nordhaus, *Economics* (New York: McGraw-Hill, 1985), p. 275.

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Fed would have to transfer huge amounts of currency to individual banks and call upon the Bureau of Engraving and Printing to increase the production of new bank notes on a massive scale.

At the present time, the money supply, M1, is approximately $770 billion, while currency in circulation is $223 billion, or 29 percent of M1. Thus, if a sizable portion of the population wished to convert bank deposits to cash, the government would face a serious shortage of the supply of currency, which has a theoretical demand of over $500 billion. And that counts only the checking-account deposits, not time deposits. It might take months for the Treasury to fulfill the demands for cash. Banks and savings institutions have the legal right to impose a thirty-day moratorium on savings withdrawals, but no such restriction exists on checking-account deposits.

A moratorium on bank withdrawals may be inevitable in a liquidity squeeze as a result of another common banking practice: the mismatching of maturities between deposits and loans (what is frequently referred to as borrowing short and lending long). Checking accounts and passbook savings deposits are usually repayable on demand, while loans to individuals and businesses are repayable over long periods of time. Even the maturity dates on certificates of deposit are not usually matched with the repayment dates on loans. Harry Browne concludes: "It's possible that 90 percent of the banks in the U.S. are vulnerable to anything that could cause depositors to want to withdraw unusual amounts of money—while the banks are powerless to recover the necessary funds earlier from their borrowers."[17]

The Interdependence of Banks

Another potential weak point in the banking system is the interdependence factor. Commercial banks and savings institutions are highly interconnected with each other. Smaller regional or state banks maintain deposits and purchase CDs from larger institutions in New York, Chicago, San Francisco, and other financial centers. It is also a common practice for smaller banks and S&Ls to sell commercial loans and mortgages to large financial institutions. Sometimes a small but aggressive bank can destabilize the whole banking industry. For example, Continental Illinois Bank and SeaFirst Bank in Seattle bought oil-related loans from Penn Square Bank, a relatively unknown institution in the Midwest. When Penn Square's billion-dollar loan programs went bankrupt in the early 1980s, it caused a serious run on Continental, the eighth largest commercial bank in the United States, and destroyed the equity value of SeaFirst's shareholders. SeaFirst had to be bought out by BankAmerica, and Continental Bank had to be bailed out by the FDIC. More recently, the FDIC has come to the rescue of another large bank, First RepublicBank of Dallas. It remains to be seen what impact failed Texas banks will have on the rest of the country.

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The federal government, always aware of a potential liquidity crisis, has made contingency plans to prevent bank runs in isolated cities from spreading elsewhere. Among other control measures, the Monetary Control Act of 1980 gives the president emergency power to declare bank holidays in specific cities in the United States where bank runs may be happening.

The Engine of Macroeconomic Instability

What could bring about a wholesale banking crisis and subsequent worldwide depression? The primary force is the increasing instability in fiscal and monetary policies of western governments. The inflationary policies of the West have created a monstrous boom–bust cycle that has gradually worsened over the past thirty years. The effects have been felt both regionally and nationwide. As a result, banking and corporate balance sheets have gradually deteriorated, especially after each recession.

Nonfinancial corporation debt in the United States reached $1.8 trillion in 1987, triple the $586.2 billion total in 1976. Interest payments have risen sharply, companies' liquidity has fallen, and credit quality has suffered. As Mickey D. Levy, chief economist of First Fidelity Bank Corp. in Philadelphia, warns: "Once the economy begins to weaken, as it sooner or later will, the high levels of corporate debt will exacerbate the downturn."[18]

Government-induced inflation via the credit markets does not simply raise prices in a relatively innocuous manner; it also creates massive distortions in the macroeconomic structure. It induces billions of dollars to be spent in wasteful malinvestments in the capital-intensive industries (e.g., real estate, oil and gas, plants and equipment, durable goods, and long-term projects).[19] Such an excessive speculative boom in the capital markets cannot last and eventually must be liquidated in the contractual phase of the business cycle. In essence, too many long-term projects and durable capital goods are produced for which there is ultimately insufficient demand. While it is difficult to quantify the level of malinvestments at any time, the degree of imbalance becomes apparent during a recession.

The United States and other western nations have been willing to endure recessions, even severe downturns, since World War II, and this development has been healthy in promoting a return to a stable and permanent economic recovery. However, it is apparent that government officials are unwilling to permit a complete liquidation of the malinvestments in the economy, which would require a full-scale deflationary depression, except on a regional basis as evidenced in the oil and agricultural depressions in the Midwest. Thus, every time the country has reached the brink of a severe recession, the government has stimulated another credit expansion to avert an economic collapse. The country's leaders are apparently afraid to let market forces determine the bottom

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of the economic contraction. One gets the impression that if the Fed did not reinflate, the economy would eventually collapse as the market sought to reestablish the real time preferences of individuals.

Hayek's Rule of Monetary Acceleration

In the 1930s, Hayek argued that the only way for government to avoid an economic downturn after an inflationary expansion is to accelerate the level of monetary expansion. I call it "Hayek's Rule of Monetary Acceleration."[20] Hayek stated that "in order to bring about constant additions to capital, it would have to do more: it would have to increase [credit] at a *constantly increasing rate*."[21]

Federal Reserve policy in the 1980s may be a classic manifestation of Hayek's Rule, as reflected in figure 3. It shows the quarter-to-quarter changes in M1 from 1960 to 1988. It is clear from the chart that (1) monetary expansion is becoming more and more inflationary and (2) monetary policy is

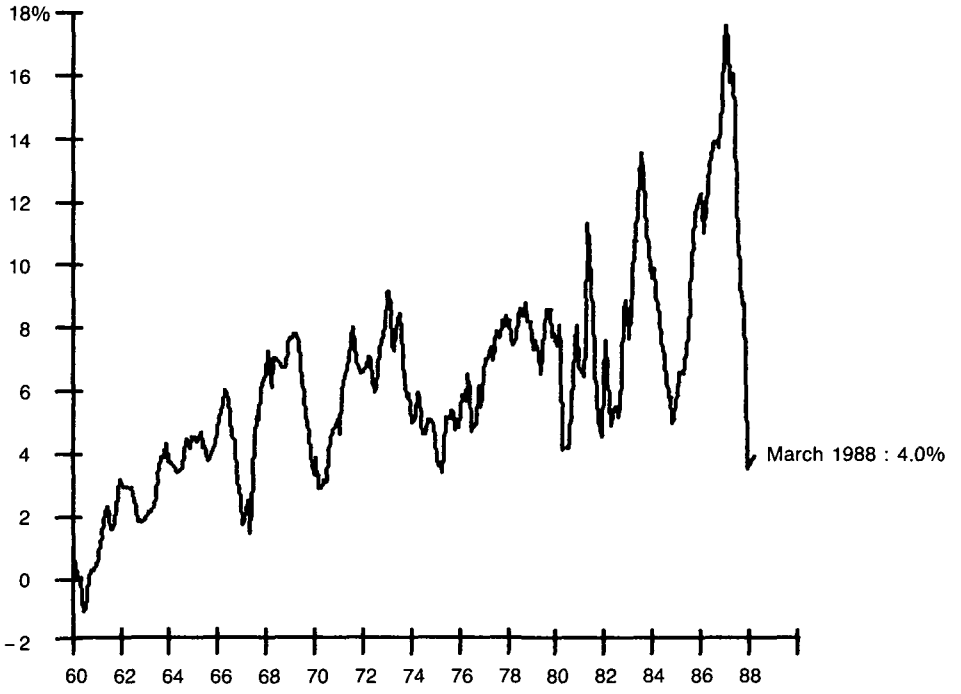

Figure 3. Quarterly Changes in U.S. Money Supply (M1), 1960–88

ource: Ed Hyman; C.J. Lawrence, Morgan Grenfell Inc., New York.

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becoming more and more volatile. In 1979, the maximum increase in M1 was 9 percent; in 1981, 12 percent; in 1983, 14 percent; in 1986, 18 percent.[22] Every attempt by Fed officials to "fight inflation" has ended abruptly a few years later as fears of a recession/depression surface. Friedman compares monetary policy to a driver turning the steering wheel of a car and hitting one side of the road and then veering over to the other side of the road. But then there is the danger that the driver will give "the steering wheel a jerk that threatens to send the car off the road."[23] Judging from recent monetary policy, the driver is becoming a reckless madman at the wheel.

The reinflation efforts of the Fed are not entirely a voluntary decision. According to Hayekian theory, the Fed cannot adopt a monetarist rule of increasing the money supply at a steady rate (3–5 percent) without causing a severe downturn in economic activity. Friedman himself admits this to be the case and advocates a gradual reduction in monetary inflation until a low monetary rule can be established on a permanent basis. But the Fed has not been so patient, apparently abandoning the use of money-supply targets in the early 1980s. The reason can be traced back to the Hayekian theory of macroeconomic disequilibrium. That is, if the central bank inflates the money supply at double-digit rates via the credit markets and then adopts a monetary rule below the previous rate of inflation, the effect will be a serious recession revealing the malinvestments in the capital-goods industries. Credit-oriented inflation presupposes a bust at some point.[24]

The longer monetary authorities maintain a "no-recession" policy, they increase the risk of runaway inflation. This is the inevitable result of Hayek's Rule. Figure 3 indicates that the United States is gradually moving in that direction. The next time the Fed panics, M1 may accelerate to a 25–30 percent rate if the M1 trend continues. This suggests the possibility that if a depression threatens the country, the central bank will attempt to inflate its way out. Moreover, given the emergency powers granted to the executive branch and the immense size of the federal government, it is quite conceivable that Washington will impose severe economic controls in an effort to contain the crisis.

The Increasing Risk of a Financial Accident

Microeconomic laissez-faire tends to undermine macroeconomic interventionism. As central banks reach higher and higher levels of monetary hyperextension, they increase the risk of precipitating a financial accident. Specifically, in the face of such extreme instability, many individuals, corporations, and institutions in the United States and elsewhere may seek to avoid a perceived disaster by acting early to eliminate debts, build a strong cash position, sell assets, buy gold, hoard currency, and so on. Anticipating deflation, some investment companies, banks, and speculators may withdraw suddenly and unexpectedly from major positions in securities, bonds, and deposits. There

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may be a run on the dollar, just as there were occasional runs on gold under the pre-1971 gold standard. There is no question that western governments are deeply concerned about the possibility of a worldwide panic in the foreign currency markets. Floating exchange rates do not eliminate speculative fever. As Sennholz states: "With floating exchange rates . . . any event, no matter how small, could trigger strong speculative movements that would cause exchange rates to fluctuate widely."[25]

Milton Friedman notes significantly that in the autumn of 1931, after England went off the gold standard, the Federal Reserve authorities feared a gold drain from the United States. Friedman records:

> Although their gold reserves greatly exceeded legal requirements and were extremely high by any absolute standard, they succumbed to something approaching panic and proceeded to take strong deflationary measures, putting up the bank rate more sharply and suddenly than at any previous time in their history—and this after two years of economic contraction. . . . True, the Reserve system reversed its policy in early 1932 and undertook moderately expansionary measures; but by then it was too late. Measures of this magnitude might easily have saved the day in 1931; by 1932 they were utterly inadequate to stem the raging tide of deflation that the Reserve system had unleashed.[26]

One wonders whether today's monetary system is that much different from that of the early 1930s. Certainly the international dollar standard makes it possible for inflation to last much longer than it could under a strict gold standard. Nevertheless, the Federal Reserve continues to adopt tight-money policies from time to time in an effort to temper inflation and bolster the U.S. dollar on foreign-exchange markets. Such actions are reminiscent of previous efforts to defend the gold standard. The Fed took strong "anti-inflation" action in 1979 under Chairman Paul Volcker and again under Alan Greenspan in 1987. Could an inordinate concern over the value of the dollar overseas lead Fed officials to repeat history by allowing monetary policy to remain too tight for too long? As in 1932, the Fed may eventually recognize its mistake, but it may be too late this time to stem the raging flood of deflation. The question becomes: have Federal Reserve officials learned from their mistakes of the past, or will they behave as recklessly as they did in the early 1930s? Having observed their actions of the past thirty years (as evidenced in figure 3), I see no reason to have confidence in them in the future. Serious if not fatal mistakes will be made over the next several years, any one of which could produce a worldwide financial panic and economic depression.

The Stock Market Panic of 1987

The October 1987 stock-market debacle was worldwide in scope. The initial downturn in the financial markets was caused by the Fed's tight-money policy

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and rising interest rates in 1987. The 508-point drop in the Dow Jones Industrial Average on October 19 was a classic panic, reflecting a series of bad economic news, the herd-like instinct of technical-trading systems, and ultimately the complete loss of confidence in the financial system by individual investors and institutions. But what was even more disturbing was the liquidity crisis that hit the day after on October 20, an event hardly publicized until weeks later. *The Wall Street Journal* reported:

> Phone calls started pouring into officials at the Big Board and the Federal Reserve Bank of New York. Angry securities dealers reported that foreign and U.S. regional banks were cutting back credit to the securities industry. Bankers Trust told Wall Street firms that it would stop extending unsecured credit—loans not collaterized by assets.
>
> Executives at one big Wall Street securities firm were shocked when another U.S. bank Tuesday refused to deliver promptly $70 million in West German marks that it had sold to the firm in a foreign-exchange trade. Apparently, the bank feared that it might not be paid promptly—if at all—for the marks. . . .
>
> After learning of the credit squeeze facing Wall Street, Messrs. Greenspan and Corrigan [president of the Federal Reserve Bank of New York] feared that something far worse than a stock-market panic might be in the offing. If credit dried up, securities firms could start to collapse, much as the banks did after the 1929 crash. Fed officials saw a real threat of gridlock developing in the markets: Even the simplest financial transaction might have become impossible.[27]

In short, the stock-market crash had all the markings of a financial accident that could have closed major markets indefinitely. Of course, the Fed intervened and prevented it from happening by guaranteeing the banks' unsecured credit to security dealers. But it is precisely this type of unexpected event that can trigger a worldwide panic, one that may not always be so easily resolved by monetary authorities.

Were the banks beneficiaries of the collapse in stock values? After the stock-market crash, one economist told *Time* magazine: "In the 1930s when things looked bad, people ran from the banks out of fear. In 1987 people run to the banks to put their money in, because this time the banks are among the safest things around."[28] This viewpoint is not entirely accurate, however. Many investors withdrew cash from their bank accounts following the stock-market collapse. There were reports that many banks were short $100 bills. Nevertheless, the economist has a point. If the public maintains confidence in the banking system, the possibility of another depression is remote. But if the banks are viewed with grave suspicion and inadequately prepared to handle large cash withdrawals, another economic collapse is a very real threat.

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The Increased Size of Government: Boon or Bane?

Friedman and other neoclassical economists argue that the increased size of government is a "built-in stabilizer" in preventing another depression. Table 1 demonstrates how government has grown since the Great Depression.

Federal, state, and local spending programs played a key role in sustaining the economy and were the principal reason why real GNP declined only a fraction during the 1973–75 and 1980–82 recessions. On the other hand, an expanding state can be a serious drag on the economy in terms of confiscatory taxes and bureaucratic rules for private enterprise and inefficient supply of public services. If the government becomes too large and socialistic, it can cause a permanent depression that may not be evident in the government statistics. The burgeoning state may not be reflected in unemployment figures, but underemployment and underproductivity will be evident everywhere.

Summary

The U.S. economy has been remarkably resilient over the years. In the turbulent 1970s, it was able to rebound from the energy crises, food shortages, and double-digit inflation. In the disinflationary 1980s, it has had to overcome major credit crunches, volatile financial markets, and banking crises. In short, Armageddon has been postponed.

Nevertheless, while the United States and other western countries may be depression-resistant, they are not depression-proof. They are highly susceptible to deep recessions from time to time. Governments have been remarkably successful in averting disaster over the past fifty years. One wonders if Armageddon can be postponed indefinitely. So far, the government forces of inflation have effectively beaten back the free-market forces of deflation. But while the government has won many battles, the war is not over. Macroeconomic imbalance is still very much in evidence; and it is, in fact, growing as monetary and fiscal policies become more and more precarious. As long as the financial-banking system is built on a volatile, destabilizing inflationary policy coupled with a fragile fractional reserve system, the possibility of financial chaos and a subsequent economic cataclysm should not be discounted.

Notes

1. Hans F. Sennholz, "Worse than 1929," in Sennholz, *Debts and Deficits* (Spring Mills, Penn.: Libertarian Press, 1987), p. 89.

2. Milton Friedman, "Why the American Economy is Depression-proof," in Friedman, *Dollars and Deficits* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), pp. 72–96. Originally appeared in *Nationalekonomiska Foreningens Forhandlingar* (1954).

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3. The only other economist I know who has written on this subject is Gottfried Haberler. Haberler offers an excellent review of all the major explanations of the Great Depression, including Austrian views, and concludes that another deflationary depression "is almost inconceivable," while an inflationary depression "is not unthinkable" and inflationary recession is more likely in the future. See Gottfried Haberler, "The Great Depression of the 1930s—Can It Happen Again?" in *Selected Essays of Gottfried Haberler,* Anthony Y.C. Koo, ed. (Cambridge, Mass.: MIT Press, 1985), pp. 405–26.

4. Friedman, "Why the American Economy Is Depression-proof," p. 74.

5. Ibid., p. 79.

6. Ibid., p. 89.

7. Ibid., p. 90.

8. Milton Friedman, "1929 and 1987: The Differences," *National Review* (November 20, 1987).

9. Letter from Milton Friedman to the author, dated December 23, 1987.

10. It is inappropriate to set a specific timetable as to when another depression will happen. I do not subscribe to Professor Ravi Batra's determinist views that a deflationary depression is inevitable in 1990. See his *The Great Depression of 1990* (New York: Simon and Schuster, 1987). Since economic events are determined by human action, not senseless machines, setting a specific date for an occurrence is an unscientific form of forecasting. Batra bases his prediction on past cycles. But history never repeats itself in exactly the same fashion. It is incongruous to compare the 1980s to the 1920s, year for year, as Batra does. There are similarities, but there are also major differences, such as the size of government. Strangely, despite Batra's claim that the depression is inevitable, he recommends that it can be avoided by the government imposing a wealth tax and rescinding the Reagan tax cuts. But the reduction in marginal tax rates may have been one of the few genuinely beneficial acts by the Reagan administration. Indeed, a reversal of these tax cuts might well cause a depression.

11. Predictions of imminent depression in the 1970s were made primarily by hard-money investment writers such as: Alexander Paris, *The Coming Credit Collapse* (New Rochelle, N.Y.: Arlington House, 1974); C.V. Myers, *The Coming Deflation* (Arlington House, 1976); Douglas R. Casey, *Crisis Investing* (Los Angeles: 76 Press, 1979); and Howard J. Ruff, *How to Prosper during the Coming Bad Years* (New York: Times Books, 1979).

12. Friedman, "Why the American Economy Is Depression-proof," p. 94. Friedman's estimates of unemployment are based on the U.S. civilian labor force in the early 1950s. At the beginning of his lecture (p. 72), he also refers to industrial production, GNP, prices, interest rates, and other data as a means of measuring economic activity.

13. Figures are based on data from *Historical Statistics of the United States: Colonial Times to 1970* (Washington, D.C.: U.S. Department of Commerce, 1975).

14. Milton Friedman and Anna J. Schwartz, *The Great Contraction: 1929–1933* (Princeton, N.J.: Princeton University Press, 1963), pp. 3–5.

15. Friedman, "Why the American Economy Is Depression-proof," p. 75.

16. The 100 percent reserve proposal is discussed in Friedman, *A Program for Monetary Stability* (New York: Fordham University Press, 1959), and Henry Simons, *Economic Policy for a Free Society* (Chicago: University of Chicago Press, 1948). To my knowledge, the 100 percent reserve concept has not been discussed recently by monetarists.

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17. Harry Browne, *Why the Best-Laid Investment Plans Usually Go Wrong* (New York: William Morrow, 1987), p. 348. Browne and his associate, Terry Coxon, have outlined an intriguing "deflation" scenario in their book, *Inflation-Proofing Your Investments* (New York: Morrow, 1981), pp. 59–83.

18. "Borrowing Binge: Takeover Trend Helps Push Corporate Debt and Defaults Upward," *Wall Street Journal*, March 15, 1988.

19. Studies on macroeconomic disequilibrium can be found in F.A. Hayek, *Prices and Production*, 2nd ed. (New York: Augustus M. Kelley, 1935); Murray N. Rothbard, *America's Great Depression*, 4th ed. (New York: Richardson and Snyder, 1983); and Gerald P. O'Driscoll, Jr., and Mario J. Rizzo, *The Economics of Time and Ignorance* (New York: Blackwell, 1985).

20. Hayek, *Prices and Production*, 2nd ed., pp. 148–52. See also Hayek, *Monetary Theory and the Trade Cycle* (London: Jonathan Cape, 1933), pp. 212–18.

21. Hayek, *Prices and Production*, p. 150.

22. Friedman prefers to use the broader-based M2, which includes money market funds, as a more consistent measure of monetary policy. He argues that since the introduction of money market funds in the late 1970s, M1 has become a misleading and narrow indicator of the money supply. M2 does not show the increasing volatility of monetary policy, although it does confirm the inflationary bias of the Fed. Interestingly, Friedman used M1 figures without reservation in his 1984 book, *Tyranny of the Status Quo* (San Diego: Harcourt Brace Jovanovich), to demonstrate the Fed's inflationary bias. I think his criticism of M1 as a narrow definition of money is valid, but M1 still reflects the direction of Federal Reserve policy, and it therefore cannot by ignored.

23. Friedman, *A Program for Monetary Stability*, p. 23.

24. Hayek, *Prices and Production*, pp. 148–49.

25. Hans F. Sennholz, *Age of Inflation* (Belmont, Mass.: Western Islands, 1979), p. 142. Chapter V, "The American Economy Is Not Depression-proof," is a critique of Freidman's 1954 lecture.

26. Friedman, "Why the American Economy Is Depression-proof," p. 81.

27. James B. Stewart and Daniel Hertzberg, "Terrible Tuesday: How the Stock Market Almost Disintegrated a Day after the Crash," *Wall Street Journal*, November 20, 1987, p. 23.

28. "The Crash," *Time*, November 2, 1987.

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The Efficient-Markets Hypothesis and Entrepreneurship

E.C. Pasour, Jr.

The entrepreneur is a key figure in the market economy. In a dynamic economy, ideas, products, and services are constantly changing. Entrepreneurship, broadly defined, refers to actions of individuals as they strive to cope with constantly changing market conditions.[1] When viewed in this way, all market participants—consumers, producers, and investors—engage in entrepreneurial activity.

Despite the crucial role of entrepreneurship in the market process, the entrepreneur is often neglected in economic theory.[2] A good example of this neglect is the efficient-markets hypothesis (EMH) of financial investments. This theory holds that the individual investor cannot outwit the market because all available information is already incorporated in stock prices.[3] The efficient-markets approach taken to its logical extreme "means that a blindfolded monkey throwing darts at a newspaper's financial pages could select a portfolio that would do just as well as one carefully selected by the expert."[4] The implication is that a buy-and-hold strategy is as good as any other and that there is no scope for entrepreneurial activity in financial markets.[5]

Insights from the Austrian theory of the competitive market process are used in this article to show that the role of the entrepreneur in investment decisions is similar to that in other spheres of economic activity. Entrepreneurial opportunities exist whenever markets are not perfectly coordinated. Hence, it is argued, there is scope for entrepreneurial activity in financial markets just as there is in other markets. In a world of uncertainty and costly information, the pinpointing of economic inefficiencies is found to be just as difficult in financial markets as it is in all other markets. Since the EMH is a version of the zero-profit theorem of competitive equilibrium in the conventional theory of the firm, it is argued that shortcomings of the EMH are similar to those of other long-run competitive theories that focus exclusively on equilibrium outcomes while ignoring the entrepreneurial market process that generated those

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outcomes. The conclusion is that neither the dart-throwing monkey nor any other automaton is a good substitute for the entrepreneur in investment markets where relative prospects for different assets are constantly changing. Before specifically considering the role of entrepreneurship in financial markets, the reason for the neglect of the entrepreneur in conventional economic analysis is briefly analyzed.

The Neglected Entrepreneur

The entrepreneur seldom appears in contemporary economic theory. A check of the index of economic theory texts will show that most make few or no references to entrepreneurship.[6] Why is the theoretical firm "entrepreneurless"?[7]

Neglect of the entrepreneurial role in formal economic analysis quite likely results from the preoccupation of economic theory with final equilibrium positions.[8] If markets are in equilibrium, economic activity is perfectly coordinated and there is no scope for profit-seeking activity. In this situation, a blindfolded monkey *can* handle the entrepreneurial function, for it has no duties. When the decisions of all market participants perfectly mesh, there are no profit opportunities. However, it is difficult to even conceive of a world in which everyone's plans are perfectly compatible and no changes are ever expected. In reality, information about the uncertain future is always imperfect and market participants constantly act to improve their lot. Thus, disequilibria are pervasive in a dynamic economy and entrepreneurship can be visualized as the ability to deal with these economic maladjustments.[9] I would not expect the plans of all market participants to be mutually compatible; but if they were, the economic world and all its transactions and contractual relationships would differ radically from those of our familiar economy.

Spotting opportunities for gain, initiating actions, and reaping the consequences of actions taken are all functions of the entrepreneur. In the competitive entrepreneurial market process, the pattern of decisions at any moment is different from the patterns of earlier periods as market participants become aware of new opportunities.[10] Consumers, producers, and investors generate a flow of information as they test their plans by engaging in market activity. Indeed, the market process itself consists of the systematic plan changes generated by the flow of information created by market participation.

Neglect of the entrepreneur in economic theory is also related to the way in which the equilibrium approach abstracts from knowledge problems. The conventional approach in economic analysis assumes that information and transactions costs "are such as to provide the conditions that are required for an economic equilibrium."[11] In this approach, emphasis is on the problem of converting resources into outputs, assuming that information on means and ends

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is *given.*[12] If information is given, success hinges on the decisionmaker's proficiency in mathematical calculation, and there is no scope for entrepreneurship. That is, if there are a given number of economic alternatives with known costs and outcomes, the economic decision is reduced to calculation. In reality, of course, means and ends are not given, and the entrepreneur must attempt to discover market opportunities and cope with constantly changing economic conditions. Thus, making the right decision calls for much more than correct mathematical calculation. It requires a shrewd or lucky assessment of present and future conditions.[13] The constant entry (and exit) of firms providing goods and services that run the gamut from fast-food to electronics is a manifestation of individual attempts to profit from constantly changing market conditions—these ventures are entrepreneurial testings of the waters.

The entrepreneur's profit and loss calculations are based on subjective estimates of future market conditions. These calculations are not the same as profits and losses for tax purposes and are not open to examination on the part of an outside observer. This point will be returned to later when it is shown why average returns to entrepreneurship may have little effect on incentives of the individual entrepreneur to engage in market activity.

Entrepreneurial Opportunities and Efficient Markets

An efficient market has been defined as one in which prices always fully reflect all available, relevant information and in which adjustment to new information is virtually instantaneous.[14] The efficient-markets theory is the formal statement of market efficiency in securities markets. The strong EMH implies that markets are always in equilibrium—in which case profit-seeking activity will be unrewarded and even penalized by positive transactions costs. The implications of the EMH are demonstrated by a well-known academic story about a finance professor and two of his students.

> The finance professor . . . was convinced that markets were always perfectly efficient. When he and the students spotted a $10 bill lying on the street, he told them to ignore it. If it was really a $10 bill, he reasoned out loud, someone would have already picked it up.[15]

Since uncertainty permeates markets, however, there is scope for entrepreneurship. Indeed, Israel Kirzner maintains that the essence of entrepreneurship is the alertness of market participants to profit from opportunities that others pass by.[16] Entrepreneurship in this sense is an equilibrating force that involves the grasping of opportunities that have somehow escaped notice. For example, perceived differences in intertemporal or interspatial demands offer the possibility of pure arbitrage profits. These profits might be viewed as a "sure

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thing"—similar to a $10 bill lying in the street. In reality, however, the arbitrageur is subject to uncertainty, and when transactions are not instantaneous, expected conditions may not materialize, resulting in losses.[17]

Even seemingly instantaneous transactions sometimes are not sure bets—entrepreneurs may face unexpected freezing of assets, bankruptcy filings, deaths, wars, and frauds such as bad checks and empty oil tanks. Moreover, no contract is so complete that it provides smooth settlement under all unanticipated conditions. The conclusion is that though entrepreneurial profit opportunities exist, they are uncertain. Consequently, there is a role for alert entrepreneurs in financial markets just as there is in all other markets. The scope for entrepreneurship in financial markets manifests itself in many different forms—from actions by individual investors to those of financial analysts. Financial markets are constantly changing and never perfectly coordinated. If individual plans are to be fully compatible, actions must be based on the same set of expectations.[18] Yet, actions of market participants suggest that their expectations about future supply and demand conditions vary widely. Indeed, every investment transaction that takes place is between two parties with opposite expectations. These differences in expectations are an important part of the incomplete coordination in financial markets that provides scope for entrepreneurship. The conclusion is that time and uncertainty pose problems and create opportunities for the entrepreneur in financial markets as in other markets.[19]

In the EMH view, profit opportunities in financial markets are considered to be "anomalies."[20] If all available information is already incorporated in stock prices, any remaining profit opportunity is viewed as a paradox. However, profit opportunities should be viewed, not as anomalies, but rather as expected outcomes of a competitive entrepreneurial market process. In the competitive market process, it is to be expected that some decisionmakers will lose and some will gain. Indeed, the market is a profit *and* loss system. Entrepreneurship represents an attempt to create or discover and thereby take advantage of profit opportunities not yet noticed by others. Since entrepreneurial profits in the market process occur only where there are maladjustments, it is misleading to speak of a "normal" rate of profit.[21] Profit and loss are phenomena representing deviations from "normalcy" and have no place in equilibrium. Moreover, there is an inherent tendency in the competitive process for profits to disappear. Profits are a permanent phenomenon only because new maladjustments appear daily.

The role of market prices in coordinating and transmitting information is well known, if often not fully appreciated. However, the competitive market process is useful not only in mobilizing existing knowledge but also in generating an awareness of opportunities whose very existence is unknown.[22] Again, it is to be expected that all entrepreneurs will not be equally adept either in coping with uncertainty or in discovering profitable opportunities.

The focus on the relationship between entrepreneurship and economic maladjustments highlights the idea that much knowledge affecting the individual

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decisionmaker is not explicit. As Hayek emphasizes, a great deal of information influencing market decisions is unique to individuals—especially knowledge of the particular circumstances of time and place, and "it is with respect to this that practically every individual has some advantage over all others."[23] And even explicit news requires daily interpretation by agents as it affects their abilities to cope with constantly changing economic conditions.

There is no way to know in advance which entrepreneurial ventures will be successful. The entrepreneurs who develop and invest early in budding growth companies are likely to receive high profits. The risk is high, but so are the prospective returns *if* the venture is successful. Investors in successful new ventures will be amply rewarded. The expectation of profits associated with the production and distribution of new products and services is the inducement that lures capital onto untried trails. The profiting from the creation of value where none existed before is the very essence of the entrepreneurial market process. Indeed, profits are determined primarily by the extent to which the entrepreneur has correctly anticipated future uncertain market conditions.

The preceding discussion, however, does not suggest that there is a formula to "beat the market." The financial investor faces a problem similar to that of other entrepreneurs in coping with problems created by time and uncertainty. In entrepreneurial choice, there is no meaningful way to describe future market outcomes objectively. When future values of economic magnitudes are treated as random variables describable by a subjectively assigned probability distribution, there is an implicit assumption that we know the future—at least the form of the probability distribution that describes it. In this approach to entrepreneurial activity, our ignorance of the future is effectively replaced by an assumption of knowledge.[24]

The Elusive Nature of Economic Efficiency

Many attempts have been made to identify market inefficiencies, where an efficient market is defined as one in which prices fully reflect all known information. However, efficiency is an elusive concept in financial markets as it is elsewhere.[25] Market participants rationally cannot be expected to base their decisions on all available information. As Stigler states: "Information costs are the costs of transportation from ignorance to omniscience and seldom can a trader afford to take the entire trip."[26] A problem arises in identifying inefficient markets under real-world conditions because of uncertainty, imperfect knowledge, and costly information. A meaningful efficiency test has not been devised even under static neoclassical conditions where a defensible criterion of efficiency must be based on an "appropriate amount" of information.[27] The amount of knowledge acquired by market participants hinges on the perceived costs and benefits. Thus, the failure of market participants to become better

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informed in markets where they are evaluating *perceived opportunities* might merely mean that the expected costs exceed the benefits.[28] The discovery that all known information is not fully reflected in prices requires resources, including mental efforts and attention, so it does not necessarily mean that markets are inefficient. The conclusion that a financial market is inefficient implies that an attainable alternative can better cope with real-world uncertainty. As critics of the "nirvana approach" have persuasively argued, however, all institutional arrangements are imperfect when measured against an idealized norm.[29]

The problem of specifying the "optimal" amount of market information is even more complicated under dynamic economic conditions of the real world where a crucially important role of the entrepreneur is to make consumers aware of available but unperceived opportunities. In this case, information about an economic good cannot be considered as a complementary ingredient that might, in principle, be purchased separately from the good.[30] It makes no sense to talk about consumers' demand for information on an investment alternative about which they know nothing. Much information about financial investments, for example, is concerned with making the consumer either aware of unknown investment opportunitites or of unperceived aspects of already known investment alternatives. Thus, the problem of identifying information inefficiencies under dynamic conditions wherever increasing knowledge has to be called to people's attention is even more intractable than in the static neoclassical approach.

There is a subtle distinction between market inefficiency and the existence of profit opportunities.[31] The fact that an outside observer cannot identify market inefficiency under real-world conditions of uncertainty and costly information does not imply a lack of entrepreneurial opportunities. At any given time, as previously stressed, profit opportunities in a dynamic economy are likely to exist because individual plans are not perfectly coordinated. This conclusion is consistent with the evidence in financial markets: "The sum total of all the evidence that has been accumulated to date suggests that discrepancies do, in fact, exist from time to time and from security to security. There is no theoretical reason why an investor cannot do a superior job of analysis and profit thereby."[32]

Entrepreneurship—Financial versus Other Assets

Assets of all sorts (including farms, forests, houses, shares of companies, fixed-interest securities, and even property such as precious metals, jewels, and antiques) compete with each other as investments. There is a continual endeavor by owners to sell those assets whose prospects the market values more highly than the owner does and to buy those in which the market valuation is less than the owners'.

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Asset markets are "inherently restless," as the relative prospects for various types of assets are constantly changing in a dynamic economy.[34] Historically, it was widely believed that common stocks scored relatively favorable long-run returns and were an effective hedge against inflation. In the 1970s, however, common stocks underperformed long-term bonds, while gold, stamps, other nontraditional investments, and farmland produced above-average returns.[35] The situation changed dramatically during the first half of the 1980s, with stocks performing much better than gold and farmland.

Even within the stock market, industries perform quite differently from one another. Over the 1943–73 period, the S&P Industrial Index increased twelve-fold, but this average consisted of widely varying industry performances. Office and business equipment stock increased 145 times and stock of the electronics industry by almost 69 times. At the other extreme, stock prices in the lead and zinc industry were less than two times beginning levels, whereas those of sugar and textile apparel industries were barely three times higher.[36] Even in shorter periods, industries perform quite differently from each other. Consequently, whether one is considering different types of assets or stock prices in different industries, outcomes are likely to vary widely and past results are not always good predictors of future performance. It is the nature of speculative assets that their relative prices will be continually changing—and in unpredictable ways.

In the market process, asset owners are constantly shifting their portfolios in response to changing economic conditions. Financial markets are similar to other asset markets in that entrepreneurial profits and losses are essential phenomena. There cannot be financial markets without them. Mises stresses that profit opportunities exist even in a "retrogressing economy" where the total sum of entrepreneurial losses is higher than that of profits.[37] It might be asked why anyone would embark on an entrepreneurial venture if it is known in advance that the mathematical chances of earning profits are smaller than those of suffering losses. However, this is an incorrect mode of posing the problem.[38] When individual investment decisions are considered in the context of Knightian uncertainty, where the set of outcomes is not and cannot be determinate, the very notion of expected-value maximization is ambiguous and misleading.[39] Moreover, entrepreneurs do not act as members of a class but as individuals. Individual entrepreneurs are not concerned about the average outcome but rather about their own outcomes. "Each entrepreneur confronts a unique situaton, and he thinks he can see opportunities, can create opportunities, to make profits. He acts quite simply because he thinks he can win."[40]

How can one test the efficacy of entrepreneurship? A public measure of the effectiveness of an action after the event when the outcome of an action is known is useless as a guide to entrepreneurial choice.[41] The action chosen is based not on circumstances of publicly agreed character but rather on the individual's own private assessment. When the subjective nature of entrepreneurial choice is

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considered, it is not surprising that entrepreneurs continue to search for profit opportunities whether entrepreneurial profits, on average, are positive or negative.

The main object of economic theory is to make the world around us intelligible in terms of human action. The entrepreneurial market-process view of economic activity is consistent with what we observe in asset markets of all types. This approach recognizes the importance and implications of uncertainty for entrepreneurial choice—regardless of whether the choices relate to production, consumption, or investments. An enterprise, an investment, a bet, or gamble has the effect of making some good thing possible--but always at a cost.[42] There is no sure way to achieve full success or to avoid disappointment in the real world where market data are shrouded in uncertainty.[43] Although there is no way to avoid the problems posed by time and uncertainty in real-world asset markets, any type of entrepreneurial venture is likely to be more successful if the decisionmaker prepares by studying the evidence.

When considered from the viewpoint of the entrepreneurial market process, investment activity in financial markets fundamentally is no different from investments in other asset markets. Random asset purchases on the part of investors in financial assets is unlikely to be a viable strategy by profit-seeking entrepreneurs. Moreover, no one can take totally predictable actions or opt out of entrepreneurship unless markets are perfectly coordinated. Since this can never happen, any model of investment activity that leaves no room for the creative elements of entrepreneurship creates confusion rather than enlightenment in attempts to understand market activity.[44] If the model omits the entrepreneur, the driving force of the market system is eliminated.

Implications—Buy and Hold versus Entrepreneurship

The EMH suggests that the individual investor cannot consistently outperform a buy-and-hold strategy. Granting that this is the case does not imply that entrepreneurial activity has no value. In the course of searching for ways to profit from "restless asset markets," investors eliminate profit opportunities for others. In the absence of such entrepreneurial activity in securities markets, there would be no action taken to reduce ignorance, exploit profit opportunities, and improve market coordination. The blindfolded monkey throwing darts at a newspaper's financial pages is not an equilibrating force in financial markets. Indeed, a considerable amount of knowledge is assumed on the part of the monkey in suggesting that entrepreneurial alternatives have been narrowed to financial-page investment choices. The astute entrepreneur, as suggested previously, must consider a broad range of investments—of which financial investments are only one type.

Regardless of whether it can be demonstrated that the individual entrepreneur can improve on a buy-and-hold strategy, entrepreneurship is a key

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ingredient in financial and all other asset markets. It is the actions of profit-seeking entrepreneurs that coordinate market activity and enable investors following a "buy-and-hold" strategy to compare favorably, on average, with those more actively engaged in market activity. The value of the trading activity that provides the benefits in highly organized financial markets is heavily discounted in the EMH. In other areas of economic activity, those individuals who accept the benefits without paying their share of the cost of providing public goods are said to pose a "free-rider problem." From the standpoint of public policy, the implication of the EMH that people generally should follow a buy-and-hold strategy—and be "free riders"—is ironic.

The preceding analysis appears to be consistent with Lachmann's analysis of capital-asset valuation and portfolio management.[45] Lachmann analyzes the asset structure of a productive enterprise as a complex network of relationships that transmits knowledge and the incentive for action from one group to another. Since the entrepreneurial decision is concerned with the making and revising of plans, a change in the composition of an investment portfolio involves the same kind of action as a change in production plan.[46] That is, the capital owner must regroup assets in response to constantly changing market conditions. Stock exchanges, by facilitating the exchange of knowledge, tend to make the expectations of large numbers of people more consistent. In this way, security markets promote consistent changes in capital investment and economic progress, since in the long run a market economy "substitutes entrepreneurs who can read the signs of the times for those who cannot."[47] The notion of the portfolio owner as a passive income recipient clearly is incompatible with the Lachmann view of market activity.

The competitive entrepreneurial process is fundamentally the same in all types of markets. The EMH is but another form of the zero-profit theorem in economics that holds that economic profit for any firm in a competitive industry is zero in the long run. In the study of financial and other markets, the preceding analysis suggests that more attention should be given to the market process generating the outcomes and less to the outcomes themselves, because they assume away informational differences and profit opportunities.[48]

In the entrepreneurial process, the search for profit opportunities eliminates or greatly reduces the prospects of success over the long run.[49] In EMH terminology, it is the search by investors for market imperfections in the belief that the market is not efficient that ultimately makes the market efficient. Potential short-run profits provide the motivation for entrepreneurial activity even though the likelihood of consistently superior entrepreneurial success is quite small.

Conclusions

An equilibrium view of economic activity is implicit in efficient-markets theory (and conventional economic theory generally). In a world in which individual

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plans are perfectly coordinated, all profit opportunities have been exploited and there is no scope for entrepreneurship.

The competitive entrepreneurial market-process view of economic activity is quite different. When time and uncertainty are taken seriously, one does not expect individual plans to be perfectly coordinated in markets for financial or other types of assets. Thus, coordination of market activity is never complete because change is always occurring. Consequently, there are likely to be entrepreneurial opportunities in asset markets of all types. Indeed, in this view, disequilibria are pervasive as entrepreneurship is exercised by consumers, producers, and investors in coping with constantly changing economic conditions. Entrepreneurship is closely linked with profits (and losses) in the competitive entrepreneurial process that is driven by the search for profits.

This article does not imply that the EMH, other zero-profit theorems, or other competitive equilibrium models are useless tools in economic analysis. It does suggest that much more attention should be given to the entrepreneurial market process that creates pressures to reduce or eliminate economic profits. It is the entrepreneurial search for profits that gives meaning to the zero-profit idea of equilibrium as an end product of the market process.

All entrepreneurial activity is shrouded in uncertainty, and there is no assurance that entrepreneurial activity by anyone will be successful. The EMH, at least to some extent, discounts the importance of entrepreneurship by suggesting that an automaton can substitute for entrepreneurial activity. The individual investor may or may not be able to beat the market averages. However, it is not market averages, but the entrepreneur's subjective assessment of profit opportunities that motivates entrepreneurial activity in securities markets as in other investments.

Competition means that entrepreneurs are unlikely to earn consistently superior returns. Although constantly changing market conditions suggest that there are likely to be profit opportunities, the constant striving for profit greatly reduces the chances of individual success. Clearly, entrepreneurial profit-seeking activity is a game worth winning, although in retrospect it frequently is a game not worth playing.[50] Regardless of an individual's success in the short or the long run, however, the profit-seeking entrepreneur is the key figure in financial as well as in other asset markets.

Notes

1. Ludwig von Mises, *Human Action*, 3rd ed. (Chicago: Henry Regnery, 1966), p. 254.

2. Israel M. Kirzner, *Competition and Entrepreneurship* (Chicago: University of Chicago Press, 1973).

3. "When someone refers to efficient capital markets, they mean that *security prices fully reflect all available information.*" Edwin J. Elton and Martin J. Gruber,

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Modern Portfolio Theory and Investment Analysis, 3rd ed. (New York: John Wiley & Sons, 1987), p. 361.

4. Burton G. Malkiel, *A Random Walk Down Wall Street*, 4th ed. (New York: W.W. Norton, 1985), p. 16.

5. There are different versions of the EMH depending upon the assumption made about the amounts and kinds of data reflected in stock prices. The discussion of this article, in a narrow sense, applies only to the "strong" form of the EMH, which asserts that stock prices fully reflect *all* information, public and nonpublic. In a broad sense, however, the discussion applies to all efficient market theories that concentrate on the results of the competitive market process, deemphasizing the actions of the market participants responsible for those results. For more discussion of the different versions of the EMH, see Charles P. Jones, *Investments: Analysis and Management* (New York: John Wiley & Sons, 1985), p. 422.

6. "The references are scanty and more often they are totally absent. The theoretical firm is entrepreneurless—the Prince of Denmark has been expunged from the discussion of *Hamlet*." William J. Baumol, "Entrepreneurship in Economic Theory," *American Economic Review*, vol. 58, May 1968, p. 66.

7. Mark Blaug describes the role of entrepreneurship in economics during the past two centuries, including the disappearance of the entrepreneur from the center of the stage in economic analysis. After analyzing the role of entrepreneurship in the works of Adam Smith, Richard Cantillon, Frank Knight, J.A. Schumpeter, and others, Blaug concludes: "It is a scandal that nowadays students of economics can spend years in the study of the subject before hearing the term *entrepreneur*." Mark Blaug, *Economic History and the History of Economics* (New York: New York University Press, 1986), p. 229.

There has been a great deal of work on entrepreneurship in recent years by a number of economists, many (most?) of whom are associated with the Austrian school. However, this work has had little influence on current economic textbooks where entrepreneurship is "hardly mentioned, or mentioned only in passing." Ibid, p. 219.

8. Israel M. Kirzner, *Perception, Opportunity, and Profit* (Chicago: University of Chicago Press, 1979), p. 5.

9. T.W. Schultz, "Investment in Entrepreneurial Ability," *Scandinavian Journal of Economics*, vol. 82, no. 4, 1980, pp. 437–48.

10. Israel M. Kirzner, Competition and Entrepreneurship, p. 15.

11. T.W. Schultz, "The Value of the Ability to Deal with Disequilibria," *Journal of Economic Literature*, vol. 13, September 1975, p. 829.

12. F.A. Hayek, "The Use of Knowledge in Society." *Individualism and Economic Order* (Chicago: University of Chicago Press, 1948).

13. Israel M. Kirzner, *Discovery and the Capitalist Process* (Chicago: University of Chicago Press, 1985), p. 17.

Moreover, the greater the uncertainties of the world, the greater is the possibility that profits would go to "venturesome and lucky rather than to logical, careful, fact-gathering individuals." A.A. Alchian, "Uncertainty, Evolution and Economic Theory," *Journal of Political Economy*, vol. 58, no. 3, June 1950, pp. 211–21. Reprinted in A.A. Alchian, *Economic Forces at Work* (Indianapolis: Liberty Press, 1977), p. 20.

14. James H. Lorie, Peter Dodd, and Mary Hamilton Kimpton, *The Stock Market: Theories and Evidence*, 2nd ed. (Homewood, Ill.: Dow Jones-Irwin, 1985), p. 177.

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15. Burton G. Malkiel, *A Random Walk Down Wall Street*, p. 348.

16. Israel M. Kirzner, *Perception, Opportunity, and Profit*, p. 8.

17. Murray Rothbard, "Professor Hèbert on Entrepreneurship," *Journal of Libertarian Studies*, vol. 7, fall 1985, p. 282.

18. Gerald P. O'Driscoll, Jr., and M.J. Rizzo, *The Economics of Time and Ignorance* (New York: Basil Blackwell, 1985), p. 80.

19. Ibid. pp. 57–58.

20. Burton G. Malkiel, *A Random Walk Down Wall Street*, p. 349. However, the EMH can also accommodate outcomes with some investors outperforming the market. Given a large number of investments, some can be expected to earn greater than average rates of return by chance alone. Although the long-run performance record for these investments is likely to fall back in line with average rates of return for investments of similar risk, the long run may be quite long. One statistician estimated that it would take about seventy years of data to demonstrate conclusively that an annual rate of return 2 percent above the market rate was the result of superior investment skill rather than luck. Gerald W. Perritt, "Consistent Winners: Is It Skill—or Luck" *AAIA Journal*, vol. 9, no. 9, October 1987, p. 27.

Alchian also stresses that individual behavior based on entrepreneurial motivation does not necessarily imply a collective pattern of behavior that is different from a random selection of actions. Under uncertainty, people's judgments will differ, even when based on the best available evidence. And even though no decisionmakers may be making their individual choices by tossing coins, the aggregate set of actions of the entire group may not be distinguishable from a set of individual actions, each selected at random. A.A. Alchian, *Economic Forces at Work*, pp. 24–25. Thus, the existence of seemingly superior investors is likely to be consistent with the EMH.

21. Ludwig von Mises, *Human Action*, p. 297. The "normal" rate of profit should be distinguished from the long-run equilibrium rate of interest. The rate of interest is positive because people have a positive rate of time preference and interest rates would be positive even in market equilibrium. Entrepreneurial profit, in contrast, is the residual when factors are paid on the basis of opportunity cost and is positive or negative because of uncertainty.

22. Friedrich A. Hayek, "Competition as a Discovery Process," chapter 12 in *New Studies in Philosophy, Politics, Economics and the History of Ideas* (Chicago: University of Chicago Press, 1978).

23. F.A. Hayek, "The Use of Knowledge in Society," in *Individualism and Economic Order* (Chicago: University of Chicago Press, 1948), p. 80.

24. Douglas Vickers, "The Uncertainty about Uncertainty: A Paradigmatic Comment on Professor Weiss' Theory of the Firm," *Eastern Economic Journal*, vol. 10, March 1984, pp. 71–77.

25. E.C. Pasour, Jr., "Economic Efficiency: Touchstone or Mirage," *The Intercollegiate Review*, vol. 17, fall/winter 1981, pp. 33–44.

26. George J. Stigler, "Imperfections in the Capital Market," *Journal of Political Economy*, vol. 75, June 1967, p. 291.

27. Harold Demsetz, "Information and Efficiency: Another Viewpoint," *Journal of Law and Economics*, vol. 12, April 1969, pp. 1–22.

28. Israel M. Kirzner, *Competition and Entrepreneurship*, pp. 155–59.

29. Harold Demsetz, "Information and Efficiency . . . ," pp. 1–22.

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30. Israel M. Kirzner, *Competition and Entrepreneurship*, pp. 155–59.

31. E.C. Pasour, Jr., "Economic Efficiency and Inefficient Economics: Another View," *Journal of Post Keynesian Economics*, vol. IV, spring 1982, pp. 454–59.

32. Charles P. Jones, *Investments: Analysis and Management*, p. 434.

33. George L.S. Shackle, *Epistemics and Economics* (Cambridge, England: Cambridge University Press, 1972), p. 195.

34. Ibid.

35. Burton G. Malkiel, *A Random Walk Down Wall Street*, pp. 298–99.

36. Charles P. Jones, *Investments: Analysis and Management*, pp. 334–35.

37. Ludwig von Mises, *Human Action*, p. 298.

38. Ibid., p. 299.

39. James M. Buchanan and A. Di Pierro, "Cognition, Choice, and Entrepreneurship," *Southern Economic Journal*, vol. 46, January 1980, p. 700.

40. Ibid.

41. George L.S. Shackle, *Epistemics and Economics*, p. 123.

42. George L.S. Shackle, "The Human Predicament," *Annals of the American Academy of Political and Social Science*, vol. 412, March 1974, p. 10.

43. Even as shrewd an economic analyst as Baumol attempts to distinguish between speculation and investing (William J. Baumol, "Entrepreneurship in Economic Theory," p. 18). All entrepreneurial activity, however, involves speculation (Ludwig von Mises, *Human Action*).

44. James M. Buchanan and A. Di Pierro, "Cognition, Choice, and Entrepreneurship," p. 701.

45. Ludwig M. Lachmann, *Capital and Its Structure* (Kansas City, Kans.: Sheed Andrews and McMeel, 1978).

46. Ibid., p. 98.

47. Ibid., p. 71.

48. The rational expectations hypothesis (RAT) of macroeconomics is another widely discussed example of the zero-profit theorem. Such models "typically begin with the assumptions that all profit opportunities have been exploited and all necessary information has already been acquired." In RAT, too, the market process has come to an end and there is no scope for entrepreneurial activity. Gerald P. O'Driscoll, Jr., and Mario J. Rizzo, *The Economics of Time and Ignorance* (New York: Basil Blackwell, 1985), p. 215.

49. James H. Lorie, Peter Dodd, and Mary Hamilton Kimpton, *The Stock Market: Theories and Evidence*, pp. 80–81.

50. Ibid., p. 81.

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Trade Unions: The Private Use of Coercive Power

W.H. Hutt†

In this article, I shall consider the nature and consequences of labor-union activities. There are various useful services that unions perform, but my interest will be solely with the social effects of *privately exercised coercive power* as a means to secure some of their main objectives (e.g., an increase in the remuneration of a particular group of workers).

Forms of Coercive Activities

This power is, in a broad sense, exercised in a twofold manner. First, is via physical violence and sabotage. We find personal intimidation of and assault on competitors, nonstrikers, strikebreakers, and managements; sabotage is inflicted even on nonunion competing firms. That unions can do so with impunity is due to their de facto exemption from society's normal sanctions against the private use of physical violence and sabotage.[1] Second, and even more important, unions employ a form of coercion that is less commonly regarded as illegitimate, namely, "peaceful coercion" via threats to disrupt the community's process of economic cooperation through a strike or a strike-threat.

Strike-threat power is the source of the authority unions have won in order to discipline their own willing or unwilling members. It has been used to impair the normal production and exchange rights of nonunion workers, and of course it has been used in attempts to exploit investors.[2] At times it has even been used detrimentally and ruthlessly against third parties—nonparties to union disputes—who have, in many countries, been denied the right to sue for damages. That is, union power is used not solely against those whose assets or labor are excluded from particular production operations, but against consumers and producers in the community as a whole. Third parties are adversely affected even in the absence of union actions explicitly aimed at them. The disruption of one set of activities throws into disorder the work and lives of others—sometimes a huge number of others.[3]

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Due to the fact that the strike is a form of warfare that, when resorted to, requires a strategy and maintenance of morale, it becomes essential during nonstriking, peaceful times to keep alive the war spirit: mistrust and hostility toward the enemy—"the employer."

The threat to strike—"the gun under the table" as Mises called it—like all forms of warfare, can be used for good or noble purposes. Nevertheless, even when the objective is defensible, we are forced to regard all private use of coercive power (whether by boycott or strike) as an intolerable infringement of human freedom. We should condemn the Mafia even if it could be shown that the revenues of racketeering were being used to subsidize opera, cancer research, or civil-rights movements.[4] Similarly, the strike is a form of private warfare. Victory is, as in all warfare, to the strong, not necessarily to the righteous. Yet during the present century, apologists for the unions have adopted "might is right!" as a moral principle.

A Fundamental Market Principle

A principle derived from the classical general theory of value that, I suggest, should be treated by all economists as a fundamental law is as follows: in any society, the flow of goods and services demanded by consumers and investors is *optimal* in magnitude and composition when each entrepreneur pays the owners of resources needed (for use in the production of individual outputs) *the minimum* required to entice their services from other applications or to retain them in the production of any particular output the entrepreneur is directing. Under this law, no resource would be employed in a use that consumers value less highly than an alternative use to which the resource could be put. This "law" is, I suggest, universal and subject to no exceptions! The institutions needed to insure that the pricing mechanism shall actually be operated in accordance with this principle are those that define and enforce transferable private property rights. Whether further institutions are needed to assist the process is still a matter of controversy. The antitrust laws of the United States were (under the kindest treatment of the arguments used for their enactment) intended to provide optimal assistance to the process that allocates the utilization of the aggregate stock of assets and the aggregate flow of labor services in conformity with "consumers' sovereignty."

In the sphere of labor, this economic law indicates that *the flow of real wages will be maximized,* and any degree of unproductive inequality of earning power will be *minimized,* when every worker desiring employment in any occupation is offered and accepts *the minimum* essential to secure his employment (i.e., to attract him from leisure or from alternative occupations and then retain his services).

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Notwithstanding the compelling logic of this law, the impression certainly survives that, in a free market (i.e., in a strike-free world), workers' remuneration would be forced down to pitiable levels. But we have never been treated to any rigorous support for that claim. There has certainly been a vast flood of contributions dealing with the issue of a "just" determination of the price of labor. But no consideration has been given to the really vital issue, namely, the crucial relationship of every individual labor cost in every particular project to the aggregate real income of society at large.

Investor Self-Defense

When investors recognize their potential vulnerability to strike or strike-threat coercion, they will make full allowance for whatever they judge to be the probability that union power will be used in an attempt to seize part of their capital. In assessing the value they can risk in assets to be devoted to any activity, investors will, to some extent, rely on the unions' reluctance to kill the goose that lays the golden egg or to unduly harm the goose's fertility! They will also consider the probabilities that (1) although in a society that tolerates strike-threat coercion, technological progress will be discouraged, it will not come to an end; labor-economizing and capital-saving achievements in noncompeting fields will still be raising the source of demands for most prospective outputs; (2) in spite of the depressive effects of the use of union power, aggregate income will continue to increase through continued thrift (provision for the future that normally takes the form of the net accumulation of assets); and (3) governments will find it expedient to inflate (reduce the value of the money unit), which, *when this action is unanticipated,* will have a positive production and employment effect in the short run (however disastrous it may be in the long run).

Investors today expect managements to be expert in (as far as possible) avoiding capitulations to strike-threat pressures, but they know they cannot rely on the managers being wholly successful. They simply know that the avoidance of capitulations to union power brings gains, while capitulations to particularly heavy wage demands will produce losses. In every decision to retain, replace, or provide (accumulated) assets in any productive activity, investors must (if, as entrepreneurs, they are forecasting rationally) regard union property seizures as prospective costs that reduce profitable investment in that activity. From society's angle, then, the consequences of union power so used will be that the composition of the community's assets stock will be adversely affected. In general, the most productive and wage-multiplying types of assets are the least versatile and therefore the most exploitable. Investors will, when possible, avoid such investments until, and if, through wise policy, exploitation

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by unions becomes less profitable.[5] Until that happens, the damage to the material well-being of labor as a whole is inevitable but incalculable.

Labor's Inferior Bargaining Power

I must now give some attention to the suggestion that unions wield "countervailing power." It is said that unorganized workers have "inferior bargaining power" in the determination of wage rates unless they can resort to the strike-threat. This influential fallacy was put very lucidly by the famous English judge, Lord Francis Jeffrey, in 1825, very shortly after the repeal of the British Combinations Law.[6] He said:

> A single master was at liberty any time to turn off the whole of his workmen at once—100 or 1000 in number—if they would not accept the wages he chose to offer. But it was made an offense for the whole of the workmen to leave the master at once if he refused to give the wages they chose to require.[7]

This sounds, of course, like an intolerable injustice, and so it appeared to the illustrious judge. But that word *master,* like the world *employer* today, really refers to the residual claimant on the value of what is being produced.

In the absence of monopoly or monopsony abuse, and provided there is no government or private restraint on the loss-avoidance, profit-seeking incentives, it will be to the investors' advantage that managers shall attract or retain all workers the value of whose inputs permits a marginal prospective yield above, or not less than, their predicted marginal-output values. But, if there is no monopolistic or monopsonistic abuse, a corporation will have no power to influence the wage rates that will be to its advantage to offer. Of course, the management's purely interpretive discretion in judging what wage rates to offer may well be wrong, in either direction.

If monopolistic or monopsonistic power exists, it is very easy to raise the value of inputs and/or outputs by excluding competing resources—labor or capital—from an occupation, industry, or area. But it is very difficult indeed to exploit complementary or noncompeting factors, such as capital by labor, or labor by capital.

We have already seen how the flow of capital into nonversatile or otherwise exploitable assets can be reduced when investors are served by vigilant managements and attempts are made to exploit them. For similar reasons, labor is unexploitable, unless managements can somehow suppress competing demands for the workers they acquire. The circumstances required for monopsonistic action to reduce wage rates are those that cause labor to be shut into a firm, occupation, industry, or area. This has occurred and may again occur. The most obvious example concerns what is known as the "lock-in contract," under which an employee who leaves a corporation is subject to some

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penalty, such as loss of pension rights. But if abuses of this kind are indeed important, they are easily remedied. Lock-in contracts can be declared void and illegal except when they are a protection for investments in human capital (resembling patents to protect investments in research) or unless the contract is a means of repayment of beneficial loans to the employees, such as for moving expenses, and so forth.

Nevertheless, in theory, the monopsonistic exploitation of labor is conceivable. This is not a matter of controversy. The most likely form in which such exploitation of labor might happen (other than through lock-in contracts) is where, by subtle fraud, workers are inveigled into specialized training for an occupation in which they find themselves trapped. I know of no concrete illustration of such a situation. But if it should occur, it would still not justify the private use of force as countervailing power.

Fortunately, there is one simple test for determining whether strike-threat power has countervailed an exploitation that has forced or maintained the price of labor below its free-market value.[8] The test is whether any workers not presently employed in firms paying the increased wage rates would be prepared to accept work of the same quality and quantity for lower wage rates. But after more than half a century of interest in this subject, I have discovered no case studies in which proof of previous monopsonistic exploitation has been demonstrated in this way.

Unions and Freedom

The most important freedom that is denied through union power is, indeed, that of the right of all individuals to accept any employment that they believe will improve their earnings and prospects. The "closed shop" or the "union shop"—devices that have been inflicted on managements in so many parts of the world—must appear to any detached student not only as flagrantly regressive but as an intolerable negation of individual freedom. Yet even under what are called "right-to-work laws," union power can force managements to deny the right of those persons who wish to raise their contributions to the common pool of income from doing so. Juveniles and the less fortunate adults (especially those who are initially less well qualified or those who belong to what Demsetz has called "nonpreferred groups," (such as blacks, nonwhites, Jews, ugly women, and elderly women) can be prevented by various subterfuges (such as color bars, demarcation obstacles, apprenticeship barriers, occupational licensing, and—most effective of all—enforcement of "the rate for the job"[9]) from improving their earnings and prospects of earnings.

Hence if by "union power" we mean the ability to coerce managements through the threat of organized disruption, the use of that power may enable potential strikers to engross for themselves the effective ability to achieve skills

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in, or to become "attached" to occupations that would otherwise be open to interlopers. The privileges so gained must be balanced against the detriments suffered by those who are debarred from employment at wage rates that it would have been profitable to offer in a truly free labor market and that prospective recipients believe could raise their earnings and prospects.

Unionists often claim that individuals' freedom is infringed because in any firm in which they work, they have no voice in the making of the rules to which they are subject or in the administration of those rules. Elliott J. Berg attributes to such well-known "labor economists" as J. Dunlop, Clark Kerr, F. Harbison, and C. Myers the view that workers "live in a state of perennial protest arising from the frustrations implicit in being governed by a web of rules they usually have little to do with making."[10] There is no legal or other barrier to the workers' volunteering to bear most of the risks, by accepting the residual share, if they so wish. They will then automatically have the right to make and administer all the rules under which they work, appoint all the managers, hire all the assets, and borrow all the circulating capital required. In that case, their earnings will be wages *plus* profits or *minus* losses, just as the investors' earnings are interest *plus* profits or *minus* losses. But the workers will then sacrifice the security of earnings and employment continuity for which the simple wage contract provides. It will, of course, also entail an inappropriate division of function so the majority of investors can spread their risks over many ventures, while workers who put their future earnings at risk cannot spread risks in that form. A sharing of risk and management is, however, by no means out of the question.[11] But what is really important is that the rules and their administration by the managers would then be unlikely to differ one iota from what they are with investors in the conventional sense accepting the residue.

The Employer

Unfortunately, the word *employer* suggests subordination to the "owners." But the suppliers of the assets and circulating capital are just as subordinate as the workers to the power of "consumer sovereignty." Consumers are the true "employers." A firm's assets are employed just as are the workers. The services of both are embodied in output. Investors willingly submit to the ruthlessness of market discipline. Seen from this angle, investors' acceptance of the residual share from the sale of output provides the most important form of social security for the workers that society offers. Stressing this truth does not of course imply that there is no problem of justice to individuals in the application of social discipline through managerial authority.

Union power is expressed partly through the promise of votes to or the subsidization of legislators at all levels (federal, state, and municipal) and reliance on lobby power generally. Legislation has indeed conferred upon the

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unions far-reaching immunities before the law. Second, and more seriously, it has, over the years, provided protection for union members from the competition of the underprivileged. Via minimum wage enactments plus "welfare" handouts, occupational licensing, prolonged unproductive schooling, and so on, private objectives sought through government have displaced social objectives sought through the market.

Union power, whether exercised through government or through the strike-threat, far from redistributing income from the rich to the poor, has had exactly the opposite effect. Yet opinionmakers and the public have been brainwashed into believing that greater distributive justice has been its aim and achievement. Although at times, through private coercion, some part of investors' property has been seized and squandered, backlash reactions upon the subsequent composition of the assets stock have soon exceeded the gains. Any long-run benefits that some unions have won for their members have chiefly been at the expense of their competitors—laid-off or excluded workers—and at the expense of all as consumers and investors in noncompeting fields. The system has had a formidably depressive effect upon aggregate purchasing power (as distinct from aggregate money-spending power)—i.e., it has repressed the wages flow and real income and has therefore caused creeping, crawling, chronic inflation to be politically expedient.

The media consistently provide the clearest evidence of the impoverishing process. But they seldom perceive (or attempt or dare explain) the reason. I can here usefully refer to just one example: the editorial introduction of Elizabeth Beardsley Butler's *Women and the Trades,* edited by Maurine Weiner Greenvald. In terrifying innocence, Ms. Greenvald refers to "thousands of pages of research about labor exploitation" contained in "an arsenal of ammunition for social reforms."[12] Ms. Greenvald continues by explaining that Ms. Butler "was one of the many foot soldiers in the war against industrialization."[13] For me, these passages, written in 1984, express the real problem. Ms. Greenvald takes it for granted that those private entrepreneurs whose perception of the availability of the women whom they connected with consumers, by offering industrial employment, were exploiters of the women they so helped. In fact, they were among the real philanthropists of their age, for they were raising the living standards of the women to whom they offered jobs and prospects on terms that were better than any alternatives that society could then offer.

The truth is that nearly all the activities initiatied under slogans such as "the War on Poverty" have, in fact, been poverty-creating. But will it ever be politically possible to restrain the most blameworthy poverty-creators of all (among the myriad of special interests that command governments), namely the AFL-CIO in the United States, the Trades Union Congress (T.U.C.) in Britain, and similar organizations in other countries? Yet politicians are often motivated to emulate the strike-threat system by enatcing minimum-wage rates that make it an offense to employ any person the value of whose product is less

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than the stipulated minimum. This type of constraint, whether misguided or cynically exploited, has survived after two centuries of extremely clever propaganda. The impression has been left that the low marginal productivity of the poor and hence their low incomes are to be blamed in some way on those who employ them. Everyone is allowed to believe that the poor are the victims of "exploitation" by their employers.

Sweated Labor

Early in the British Industrial Revolution, those who offered employment to the very poor came to be described by the abusive epithet, *sweaters*. The stereotype of the sweater was of a small businessman (often an immigrant and Jewish) carrying on his back a sack of materials cut to a required pattern. [14] The profession of these sweaters was that of a small entrepreneur—typically, a clothing manufacturer. Sweaters would cut the different parts, for instance, needed to make a shirt. They would then call on houses in the poorer districts and seek out suitable housewives not fully employed in household chores, and they showed these women how to sew the different parts together. Later, they again would call to collect the product and pay for the outputs. They had no means of preventing any of these housewives from obtaining better-paying jobs. Yet the opprobrium worked up against this class was enormous. The sweaters were accused not only of underpaying, but of overworking these women, all of whom voluntarily and usually eagerly accepted the contracts offered them. Indeed, these small entrepreneurs were charged with working the seamstresses they provided with jobs to an early death. British humorist Thomas Hood's famous "Song of the Shirt" offered the refrain: "Stitch, stitch, stitch" followed by the line: "It is not linen you're wearing out, but human creatures' lives!"

The term *sweated labor* became common parlance in British socialist circles, while the famous historians of the trade union movement, Sydney and Beatrice Webb, used their literary skills to keep the notion alive. It is important to remember that these greatly aligned small entrepreneurs were never accused of using tricks to prevent other entrepreneurs from entering their territories. But the antisweating movement, largely financed, in its early years at least, by factory owners who complained about "the unfair competition" of small domestic manufacturers (who were not forced, as they were, to invest in expensive machinery), has continued right down to this day.

It was really a shock to see Dan Rather on a CBS news report resuscitate the myth and dress it up in modern clothing. He staged a woman forced to work at home on piecework for a mere pittance, with total earnings well below the minimum wage rate, in spite of deplorably long hours. Dan Rather's aim was, of course, an attempt to justify legislation that prohibited people from earning any income at all unless they could produce outputs that were salable

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(to the scoundrels engaged in this kind of business) for sums equal to or exceeding the minimum specified. How the hearts of millions of CBS listeners must have bled for the poor woman portrayed and the thousands of others like her!

But the only "exploiters" of such women are governments or private organizations (such as labor unions) that impose restraints on the free-market price of labor and so destroy entrepreneurial incentives to offer better-remunerated employments to all. Every such restraint is the result of coercion—by government or by the private use of the right to disrupt (e.g., the right to boycott, to strike, or to use intimidation and violence generally for the same purpose).

Unions and Blacks

In the United States, black people have been most sedulously used by professional white and black "liberals" and unionists for their private ends. So-called liberal politicians have persuaded black voters to renounce the protection and assistance of the market and subject themselves to the mercy of the state (e.g., to the rulers of special-interest organizations).

The overwhelming majority of black leaders who attended a recent meeting of the Urban League and NAACP's Joint Summit Conference on the Crisis in the Black Family saw things differently. In a brilliant article in *The American Spectator,* William Tucker reports how "speaker after speaker recited the indictment that charges White America, once again, with consigning Black America to a permanent internal exile." Blacks, reported Tucker, "remain hypersensitive to every one of life's little frustations, particularly identifying every adverse event as some new form of 'discrimination.' "[15] Referring to the fact that about half of all black children in the United States today are "illegitimate," Tucker reports that today about three-quarters of them are being reared without the influence of a father.[16] This is a recent and still developing phenomenon. Had the conference been genuinely concerned, however, with the well-being of American blacks, they would have directed their main attention courageously to the frightening prospect this situation is creating. Instead, the blame was usually laid on those hackneyed scapegoats, "joblessness, discrimination, poor education, poor housing, and the failure of the government to give us our share."[17]

Tucker interprets this as evidence that blacks as a whole "still refuse to recognize that it is the incredibly misguided 'charity' of the welfare system that is breaking up their families." He alleges that "the welfare process" is creating "vested interests that are going to be very, very hard to dislodge" if an attempt to reverse the trend is to be made in the future.[18]

Tucker most effectively quotes George Gilder's "almost totally ignored book, *Visible Man.*" Gilder says:

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> The welfare system makes an irresistible offer to every female over the age of sixteen. The State says, so to speak, to every black girl, "If you have a baby *right now,* we will give you your own apartment, free medical care, food stamps, and a regular income over the next twenty years. If you have another baby soon after, we will increase your allotment." How many teenage girls *anywhere*—Black or White, poor or affluent—can afford their own apartment and pay their own medical expenses at age sixteen? These teenage girls . . . are not morally weak, and they are not sexually lascivious. They are simply rational human beings making the most intelligent choice on how to improve.[19]

Neither Tucker's nor Gilder's important contributions have, however, dealt mainly with what I believe to have been the most serious detriment that has brought about the current situation—namely, acquiescence in the pseudoprinciple of "the rate for the job" as a criterion for determining labor's just remuneration plus faith in the beneficence of wage rates enacted under nonmarket coercion. Had it not been for the influence of this pseudoprinciple (conspicuous in the rhetoric of the collective determination of the price of labor over the past century), blacks would, I submit, have been well on the way to enunciating a salable program developed to mitigate and solve a majority of the problems that now face them.

The National Urban League, in its report of this conference, frankly admits that "29 percent of all Black men between the ages of 20 and 64 . . . were unemployed in 1982."[20] But as to causes, the League's research director refers only to inadequate schools, high arrest rates, and proportionately high murder and suicide rates. There seems to have been a reluctance to admit that the major cause of the damage wrought on the black people was due to the unwillingness of their community to fight aggressively for well-paid employment by deliberately reducing their per capita demands for wage compensation.

To sum up, in a free society, aggregate real income is maximized and inequalities of income are minimized when every person who wishes to be employed in any undertaking is offered and accepts the minimum necessary to be attracted from leisure or from other pursuits or employments, while those who provide the services of the assets they own are also paid the minimum necessary to obtain that provision or to attract their services from other occupations.

Hence, not a solitary cent of aggregate income has ever been transferred through strike-threat pressures from investors as a whole (the providers and owners of assets) to workers as a whole (the users of assets). The consequences have clearly harmed both groups, more or less in the same proportion, with regressive consequences on the aggregate wages flow. The effects upon the internal flow of savings and the import of capital are of course important, but there has never been a better mechanism for fructifying thrift and, thus, of insuring the advance of economic development than through competition in the free market.

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One hears everywhere, however, that the political influence of the unions and the strength of the AFL-CIO and the British T.U.C. are so great that all who contemplate legislation to curb their power to deplete the wages flow are pursuing a hopelessly lost cause—a political will-o-the wisp. Certainly Parliament in Britain has tried to foster unionism by legislation that confers a monopoly of bargaining power upon a single union with the right to demand compulsory membership for all employees. This is a stark reality that we must face. And the position is not so different in the United States. But have not circumstances been creating a situation in which the great supposed lost cause can be turned into a triumphant battle cry?

The political influence of the labor unions, however, expressed largely through the federated bodies that I have been blaming (the AFL-CIO and the T.U.C.), has throughout been impoverishing in the worst sense of that term, insofar as that influence has been used especially for the benefit of union membership; i.e., it has aimed at the entrenchment of privileged employment and the protection of the union officials' "profession." In other respects, while those representatives whom the union organizations support or finance may occasionally have used their powers in an enlightened manner for the common good, as spokesmen for the unions they have pleaded for and lobbied for the most sordid of special interests; and in this role they appear to have been conspicuously unconcerned about the interests of the working class as a whole.

Notes

1. There is a large literature dealing with coercion of this type. Sylvester Petro's contributions are most important. E.P. Schmidt's *Union Power* (Los Angeles: Nash, 1973), especially chapter 9, is another outstanding analysis and exposure of the situation as it still exists today.

2. Yet investors have taken the risk of providing labor's tools—the assets that multiply the yield to human effort. Moreover, investors continuously finance replacement of materials and work in progress.

3. The New York transit strike of 1966 is an apt example. The British coal strike of 1926 was almost as disastrous for the uninvolved community as the general strike of that year.

4. W.H. Hutt, *The Strike-Threat System* (New Rochelle, N.Y.: Arlington House, 1973), p. 44.

5. Investment in that form must certainly decline relatively to investment in non-unionized activities or in more versatile—less specialized—resources.

6. Between 1799 and 1825, laws were aimed at strengthening the ancient common law against restraint of trade. Approximately forty statutes applying to particular trades or industries were enacted against monopolistic pricing and wage-rate fixing.

7. Quoted in Sydney and Beatrice Webb, eds., *History of Trade Unionism* (London: Longmans, 1920), p. 72.

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8. That is, below the wage rates that would be determined in the light of wage rates in alternative employments open to the workers concerned.

9. Sometimes called "equal pay for equal work," "the standard rate," or "comparable worth."

10. E.W. Bakke et al., *Unions, Management, and the Public,* 3rd ed. (New York: Harcourt, Brace & World, 1967), p. 19.

11. Ibid., pp. 80–82.

12. Elizabeth Beardsley Butler, *Women and the Trades* (Salem, N.Y.: Ayer Co., 1984), p. x.

13. Ibid., p. xi.

14. There was a definite tone of anti-semitism about the British antisweating movement of the nineteenth century.

15. *The American Spectator,* July 1984, p. 14.

16. Ibid., p. 15.

17. George Gilder, quoted in ibid., p. 15.

18. Ibid., p. 15.

19. Ibid., p. 15.

20. As reported by UPI in the *Dallas Morning News,* August 1, 1984.

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Notes and Replies

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Mises on the Evenly Rotating Economy

J. Patrick Gunning

The term *evenly rotating economy* (ERE) was introduced by Ludwig von Mises in order to distinguish the concept of static equilibrium used by economists from equilibrium models used by students of physical systems. In Mises' view, the mathematical model of physical science had been unwisely extended to economics by writers who had failed to recognize the essential difference between the subject matter of human and nonhuman science. This lack of wisdom was evident from the fact that when even the best mathematical economists set out to describe real economic activity, they had to resort to making assumptions or hypotheses about entrepreneurial activities. In introducing the term ERE, Mises was not proposing that economists adopt a new method of describing economic activity. He was merely proposing that economists use a more accurate phrase to describe the method they had always used but that some economists apparently failed to appreciate.[1]

The phrase *evenly rotating economy* was adopted by only one other economist of note, Mises' student Murray Rothbard. Mainstream economists continued to use the terms *static equilibrium* or *general equilibrium*. Of course, the words one uses are not crucial. Mises could have continued to use the term *equilibrium* if he had convinced his readers to interpret it according to its logical meaning and not its mathematical meaning.

The mathematical meaning of equilibrium derives from an analogy with the laws or regularities of nonhuman phenomena. An example is equilibrium in hydraulics. Hydraulic equilibrium is based on the law of gravity and describes a mathematical relationship between conditions that prevail when the system is set into motion and the conditions that prevail after some period of time when the motion stops. The logical meaning of equilibrium is based on an analogy with the logic of human action. Equilibrium in human action refers to a hypothetical point in time when a human being stops deliberating and proceeds to perform the behavior that is specified by an assumed plan of action. For an economy, the ERE is an analogous hypothetical point in time when everyone stops deliberating and proceeds to perform the behavior specified by some general plan of action.

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When equilibrium is given a logical meaning, one is naturally led to conceive of the preequilibrium actions that would be necessary for the equilibrium to be achieved. Regarding equilibrium in an individual action, one is naturally led to think about the deliberation and experimentation that precedes the point when the individual decides to stop deliberating and to carry out the plan of action. Regarding equilibrium in an economy, one is naturally led to think about the entrepreneurship that activates the "market process." In fact, the concept of equilibrium in an economy has no other use than to help the economist elucidate entrepreneurship. Equilibrium is a notion developed by the economist for the distinct purpose of delineating the entrepreneurial part of economic interaction that the economist knows by intuition (a priori) to be present. The ERE is an imaginary state in which all of the essential economic functions—production, saving, consumption, and factor supply—are performed by automatons. Unless an economist constructs an image of such a state, he finds it impossible to comprehend the full range of entrepreneurship in economic interaction.[2]

The distinctions between the logical and mathematical methods and between logical and mathematical meanings are crucial in Misesian economics. Failure to understand them is a sure route to a failure to understand the meanings of many terms and passages in Mises' economics writings. An example of such a failure is a 1985 critique of Mises' concept of the ERE in the *American Economic Review* by Tyler Cowen and Richard Fink. Cowen and Fink (C-F) correctly suggested that the ERE is an alternative to the mathematical interpretation of the Arrow-Hahn-Debreu model of general equilibrium. However, they went on to say that there are "serious inconsistencies in both the nature of the ERE and its suggested uses." They proceeded to present criticisms of four uses that they claim were proposed by Mises and Murray Rothbard.

This article is intended to absolve Mises of the C-F criticism. By means of quotations, text references, and argument, I shall attempt to show that C-F misrepresented Mises' views, that they failed to identify the fundamental use(s) that Mises claimed for the ERE, and that their own evaluation of the usefulness of the ERE is faulty. More importantly, I will argue that Mises demonstrated that equilibrium constructs, properly conceived, are not only useful but necessary. Without them, there is no way to provide a meaningful description of the most significant activity that occurs in a market economy: entrepreneurship.[3]

This article proceeds by addressing each of the four C-F criticisms in turn. In the process, Mises' ideas and the apparent reasoning behind them are identified and discussed.

Explanation, Prediction, and Disequilibrium

The first use that C-F claim Mises suggests is that "the ERE can be used to explain or 'predict' the direction of change" (p. 866). I could find no remark by

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Mises to substantiate the C-F view that Mises makes this suggestion. It is true that Mises says that complex phenomena can only be studied by abstracting from change and then introducing an isolated factor to provoke change (p. 248). But this is not the same as saying that a purpose of ERE is to explain or predict the direction of change. C-F provide no references to Mises' writings in their discussion of this point.

It is a misuse of language to employ the terms *explain* and *predict* to refer to what Mises hoped to accomplish by introducing an isolated factor to provoke a change. To most economists, these terms refer to descriptions of actual economic activity. Economists explain why the price of sugar fell and they predict the statistical rate of unemployment. In the passages where Mises discusses change, however, he is not interested in the particular consequences of a provoking factor change. He is concerned with describing a mental process that can be used by an economist to acquire a personal understanding of entrepreneurship in a market economy. The change to be provoked is a hypothetical change. The procedure Mises has in mind can be expressed in the following way. Supposing that products are related to factors of production according to the theory of imputation implied in the ERE, we want to form some hypotheses about how the relationships would change if we introduced a change in the data. Then, having done so, we want to imagine the various (entrepreneurial) actions that individuals would have to take to cause the relationships to change. Mises defines *data* as "the bodily and psychological features of the acting men, their desires and value judgments, and the theories, doctrines, and ideologies they develop in order to adjust themselves purposely to the conditions of their environment and thus to attain the ends they are aiming at" (p. 646).

The expressions used by C-F to discuss Mises' use of ERE with respect to "change"—namely, that "the ERE can be used to explain or 'predict' the direction of change" or that "the ERE is a starting point for the analysis of particular changes"—mislead the reader. They incline him to think that Mises was a positivist of some type. Mises criticized positivism.

The C-F discussion of this use focuses on the question of whether there is a tendency toward equilibrium. It consists of two parts. In the first part, they appear to criticize Rothbard and, by implication, Mises for claiming that there is a tendency toward equilibrium (p. 867). Interestingly, however, they seem to (unwittingly?) echo Mises. Mises says that we should not "disregard the fact that the market is [continuously] agitated by factors which must result in further price changes and a tendency toward a different state of rest" (p. 246).

Lest I leave the reader with the wrong impression, it is still true that C-F and Mises are addressing different issues. The preceding quotation from Mises should not be interpreted to mean that economists' efforts to predict the direction of a change in price must take "disequilibrating factors" into account. He is simply not interested in prediction. He is referring instead to the main function

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that he believed the ERE serves: the elucidation of entrepreneurial profit and loss.[4] He is saying that to recognize how entrepreneurs contribute to the determination of prices under the conditions specified in his definition of the market economy, economists must take account of the fact that prices are continually changing.

The second part of the C-F discussion addresses the view that "there is a tendency towards equilibrium in a world of frozen data" (p. 867). Mises, in discussing entrepreneurship, says: "These endeavors of the entrepreneurs would finally result in the emergence of the evenly rotating economy if no further changes in the data were to appear" (p. 329). C-F correctly point out (given their meaning of the word *tendency*) that "this implies . . . nothing about whether there is a tendency towards equilibrium in a world where the data are not frozen." But they reveal their misunderstanding of Mises when they go on to say that the Mises view would just as easily admit "a tendency toward disequilibrium." And further: "By allowing the data to change just as it does in the real world, and 'freezing' all individual learning, we can demonstrate that the economy would degenerate into a series of successively less-coordinated states of disequilibrium."

The problem with C-F's criticism is that it is based on a *mathematical* definition of equilibrium and not a *logical* definition consistent with Mises' pure logic of action. In the logical definition, the concept of disequilibrium is meaningless. To say that there is a tendency toward disequilibrium is like saying that individuals do not make choices.

Mises constructed a pure logic of action. In this logic, he sought to deduce outcomes based on the assumption that individuals choose. It should be evident that, in such a logic, there is no place for a statement that individuals do not choose. As part of his pure logic of action, Mises also constructed a logic of entrepreneurial action. This logic sought to identify and elucidate the entrepreneural discovery of the data and the acts associated with trying to profit from such a discovery by promoting exchanges. In this logic, there is no place for a statement that entrepreneurs do not discover and do not try to profit from their discoveries. Yet this is precisely what is implied by the statement that there is a "tendency toward disequilibrium," at least if one applies this statement to the ERE. Such a statement implies that the data cannot be discovered or that a profit cannot be earned by doing so.[5]

Analysis of Complex Phenomena

The second use of the ERE attributed to Mises by C-F is that the "ERE is an analytical building block or stepping stone towards analyzing complex phenomena in a world of change." I think that this is a correct though vague characterization of Mises' view. However, C-F's criticism of it fails to account

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both for Mises' own statements on the matter and for Mises' purpose. C-F claim that such a use of the ERE is open to question because "there are no prices in the evenly rotating economy." (p. 867). Mises says that the ERE "is a fictitious system in which the market prices of all goods and services coincide with final prices" (p. 247).

To C-F, it is a contradiction to maintain that we can learn something about how prices are formed by employing an image which, by definition, assumes that the prices are already formed. Their discussion in this respect ends with the rhetorical question: "How can an imaginary construct illuminate an institution [the system of markets and prices] that performs absolutely no function within that construct?" (p. 868).

C-F's misinterpretation on this issue is due to their failure to grasp the fact that the ERE was employed by Mises solely to elucidate the entrepreneurship that causes factors of production to exist in a causal relation with wants. Mises' use of the ERE to elucidate entrepreneurship can be most forcefully shown by an analogy to a one-person choice. In describing and understanding the nature of an individual's choice, we would have to have in mind the ends that are sought. It would be senseless to attempt to describe or understand the deliberation process that precedes a choice, the learning that occurs, the experimentation that an individual carries out, and so on, unless we had in mind a set of ends the individual was trying to achieve. Another way to put this is to say that to elucidate deliberation processes and so on, we must have in mind an end point, namely, a point at which a choice is made.[6] With this idea in mind, we proceed to work our way back, so to speak, to the activities that an individual can perform to cause this end point to be reached. It is difficult to conceive of any other way to proceed. If one merely described physical behavior and labelled it "deliberation," "learning," and so on without at the same time telling the end that was sought, his descriptions would be impossible to assess.

Similarly, it is essential for the economist to have a series of relative prices, production operations, and a final distribution of goods in mind when he begins to explore the signalling and trading processes through which the prices, production, and distribution are achieved. These prices, and so on are merely the economist's abstract conceptualization of the ends of "consumers" and of the consequences of the entrepreneurial decisions that enable the ends to be met in some measure.

It is true that one can hardly conceive of the ends of all consumers and of the many interdependent entrepreneurial decisions that cause them to be achieved. Nevertheless, without such a conceptualization, the whole notion of economic interaction itself dissolves into a fruitless analogy with nonhuman systems. The burden of the economist is to devise means of making such conceptualizations, however imperfect they must be.

Since Mises regarded the ERE as the outcome of a choosing process that involved entrepreneurship, it is easy to answer C-F's rhetorical question.

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Entrepreneurship, the cause of the prices, can only be understood if we have in mind a pattern of prices, production operations, and a final distribution of goods. The ERE is such a pattern.

Analysis of Changes

The third use to which the ERE has been put, according to C-F, is that the "ERE is a starting point for an analysis of the effects of particular changes" (p. 866). They take this to refer to the analysis of the effects of an exogenous shock upon the economic system. I noted previously that Mises speaks of introducing an isolated factor to provoke change, but that by change he meant a change in desires, value judgments, and so on. However, I do not think that a change in these data is what most economists mean by an exogenous shock. Whereas a change in the data suggests the logical method, an exogenous shock suggests the mathematical method.

The mathematical method of representing equilibrium assumes that individuals operate according to maximization rules. Such operations cause "economic variables" to be deducible from a given set of parameters. An exogenous shock is synonymous with a change in a parameter. The effect of such a shock is a change in the values of the variables. The solution describes the direction and/or size of the change in values.

The logical method sees equilibrium as a relationship between products and factors of production—a theory of imputation. It is assumed that the relationship is a consequence of profit-seeking entrepreneurial choices, but the equilibrium reveals nothing about the nature of those choices. To discover what they are, the economist must imagine some change in the *data*. Then, by projecting himself into the positions of different individuals who buy the products and supply the factors of production, he must ask how he could avoid a loss or earn a profit. It should be obvious that to employ the term *exogenous shock* to refer to a change in the data in this context is likely to be misleading.[7]

Beyond this, C-F make a mistake when they introduce an example to illustrate what they take to be the contradictory nature of ERE. Their example is the Austrian theory of the trade cycle. They say that "using the ERE analysis as a starting point for the analysis of monetary intervention (for example, Rothbard and Mises' business cycle theory) involves the contradiction of superimposing an increase in the money supply upon an essentially moneyless world" (p. 868). This statement cannot be squared with Mises' trade-cycle theory. In a section where he introduces a discussion of a change in the quantity of money, Mises clearly acknowledges that money is incompatible with the ERE (p. 417). It would be stretching the imagination to suppose that having said this (and having referred the reader to an early section where the same point was made), Mises would proceed to superimpose an increase in the money

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supply upon an essentially moneyless world. The fact is that Mises superimposes the increase on a changing world in which money is already employed as a means of signalling and reckoning. In short, Mises does not use the ERE to describe the trade cycle.

In considering how Mises described entrepreneurship in the study of economic change, one should be careful not to conclude that this was Mises' only conception of entrepreneurship. In the theory of economic change, one begins with a starting point in which certain factors and goods are already being produced and used, in which certain markets are already created, and in which data are defined by the economist and do not need to be discovered. The range of activities that are involved in reaching an end point under such circumstances is obviously limited. There is no reason, however, why the praxeological method should limit itself to such a rigid starting point. One may start with a situation in which there is a complete absence of markets, an absence of previously produced factors, and an initial absence of knowledge of others' wants, abilities, and knowledge. Entrepreneurship can then be imagined to identify the data, create markets, and produce factors and goods such that an ERE is eventually established. Mises does not emphasize this point and most of his discussions of entrepreneurship are limited to the context of economic change. Nevertheless, the point is evident from his remarks.[8]

ERE as a Foil

The fourth use to which C-F claim Mises and Rothbard put the ERE is as a "foil." The word *foil* is ambiguous. Mises does not use the term so far as I know. As a result, I was unable to find the proper reference for the C-F criticism.[9] The term itself seems to suggest that Mises uses the ERE as a sort of strawman against which to pit his own approach to economic phenomena. This interpretation, however, is not consistent with the C-F discussion. C-F's discussion uses the term *foil* in the same sense that Mises uses the term *argumentum a contrario*.[10]

The argument that C-F make claims in essence that Mises' *argumentum a contrario* is nonsensical: "If, as Mises claims . . . the ERE has no human action, then we cannot claim there is a tendency towards equilibrium, since this would imply the nonsensical conclusion that there is a tendency for human action (and human institutions) to disappear" (p. 868). The error here is C-F's failure to recognize that the ERE is regarded by Mises as an imaginary or hypothetical *consequence* of entrepreneurial choices, not as a descriptive or mathematical model. There is nothing nonsensical about saying that economists can better (or only) understand entrepreneurship by contrasting a state in which there is a motivation for entrepreneurial action with an imaginary state in which the motivation is no longer present. It is, of course, nonsence to argue that the

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state in which entrepreneurial action is present is like the state in which it is not present.

C-F are not alone in their misinterpretation of Mises' *argumentum a contrario*. O'Driscoll and Rizzo (1985) say that "from Mises' perspective, the exact equilibrium construct merely provides us with a clue as to why certain results do not obtain. Hence it is capable of only negative prediction. It does not elucidate the actual processes by which those states may be achieved." They go on to say that Mises' method "does not permit us to offer a logically sufficient explanation for the 'failure' of actual processes" (p. 82).

The language O'Driscoll and Rizzo (O-R) use, like that of C-F, conjures up the image that the nature of the processes through which equilibrium is achieved is a *scientific* mystery. This language misleads the reader about Mises' intent. In using the ERE as an *argumentum a contrario*, Mises was not interested in determining why equilibrium was not reached, in providing a clue as to why, or in making predictions. His sole concern with the equilibrium model was to employ the model's implicit theory of imputation as a framework for identifying entrepreneurship.

Rather than a foil, the *argumentum a contrario* is like an infrared lens that makes it possible to identify a phenomenon that cannot be identified in any other way. To be sure, it does not guarantee that what one sees is what he believes he sees. But without the lens, he would be unable to see anything at all.

Conclusion

In my view, Mises' *Human Action* is at the same time the most important economics book of this century and the easiest to misunderstand. Among its many contributions is its correct statement of the meaning and usefulness of the concept of static equilibrium, or evenly rotating economy, in economic analysis. Mises' argument was that the ERE is an end point—a complex relationship between consumers' subjective values and factors of production.

Without a theory that contains such an end point, it would be impossible to make sense of the entrepreneurial actions and the consequences that are so readily observable in a market economy. Our observations would be facts without a theory—or, more correctly, they would be meaningless accounts. We could not distinguish them from any other observations. In short, whenever we say "X is a resource" (or "C is a cost"), we must, in the backs of our minds, have an idea of the wants X will help to satisfy (the want satisfaction that must be foregone) and the entrepreneurial activity that caused the relation between X (C) and the want to come into existence. "X is a resource" has no other meaning. And we could not properly say that X is a resource unless this is what we mean.

The ERE is simply a means of conceptualizing a combination of exchanges. As we attempt to work our way back from the ERE, we must identify the ultimate

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ends, the intermediate ends, the separate exchanges, and "means of coordination" within a "real" market economy. As the entrepreneurial activities that might lead the ends to be achieved are identified, a theory of imputation unfolds in which one learns the wide range of factors of production that contribute to the satisfaction of wants. One also learns how these factors would not exist and the wants would not be satisfied without entrepreneurial action.

The fundamental error made by C-F might be stated as follows. They were interested in the question of whether the ERE is a useful model of prices, outputs, and other variables typically identified in the simultaneous equations of the mathematical models of static equilibrium. They interpreted the ERE as an abstrct representation of a real economy which contained these variables. Then they asked whether it was a useful abstraction. Their answer was no, the same answer that Mises himself gave. They neglected to ask, however, whether the ERE is a useful reference for helping one identify and describe the entrepreneurial actions that are intended to cause exchanges to occur. They also neglected to identify this purpose as the only one that Mises felt the ERE was equipped to help the economist accomplish. Without the ERE, Mises argued, there would be no way to identify and discuss entrepreneurship. Failing to identify why Mises used the ERE and focusing on a purpose for which they believed it should be used, they completely misinterpreted and misrepresented Mises' views.

Mises warned users of the ERE. He pointed out that the method of imaginary constructions, of which ERE is an example, "leads along a sharp edge: on both sides yawns the chasm of absurdity and nonsense. Only merciless self-criticism can prevent a man from falling headlong into these abysmal depths" (p. 237).

Notes

1. See Mises, *Human Action,* 1966, p. 239.

2. The following passages from Mises (1966) are significant in supporting this interpretation of the distinction between the mathematical and the logical methods:

> The mathematical economists [who have adopted the mathematical method as opposed to the logical method] disregard dealing with the actions which . . . are supposed to bring about the evenly rotating economy. (p. 250)

In other words, they disregard entrepreneurship.

> Now, the mathematical economist does not contribute anything to the elucidation of the market process [i.e., the process in which entrepreneurship is the driving force]. He merely describes an auxiliary makeshift [the ERE] employed by the logical economists as a limiting notion, the definition of a state of affairs in which there is no longer any action and the market process has come to a standstill. That is all he can say. What the logical economist sets forth in words when defining the imaginary constructions of the final state of rest and the evenly rotating economy and what the mathematical economist himself must describe in words before he embarks upon his mathematical work, is translated into algebraic symbols.

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> Both the logical and the mathematical economists assert that human action ultimately aims at the establishment of such a state of equilibrium and would reach it if all further changes in the data were to cease. But the logical economist knows much more than that. He shows how the activities of enterprising men, the promoters and speculators, eager to profit from discrepancies in the price structure, tend toward eradicating such discrepancies and thereby also toward blotting out the sources of entrepreneurial profit and loss. . . . This is the task of economic theory. . . . The problem is the analysis of the market process.
>
> The mathematical method is at a loss to show how from a state of nonequilibrium those actions spring up which tend toward the establishment of equilibrium. (pp. 355–56)

3. I find it quite puzzling that Israel Kirzner has not emphasized the logical method and the evenly rotating economy in his writings on entrepreneurship. It is beyond the scope of this article to enter into a discussion of the nature of entrepreneurship. Suffice it to say at the moment that in the author's view, there appears to be a substantial difference between the method of identifying entrepreneurship used by Mises and that used by Kirzner. See Gunning (1988).

4. Mises says this repeatedly throughout his book. I think, however, that his most complete statement is on p. 248.

> These insoluble contradictions [that change is eliminated in the ERE, that the ERE is not peopled with living men and that real action does not correspond to the ERE], however, do not affect the service which this imaginary construction renders for the only problems for whose treatment it is both appropriate and indispensable: the problem of the relation between the prices of products and those of the factors required for their production, and the implied problems of entrepreneurship and of profit and loss.

In short, Mises sees ERE as a starting point for a theory of imputation in which the principal consideration is profit seeking by the entrepreneur.

5. This interpretation appears consistent with the only reference I could find to disequilibrium in Mises' *Human Action* (p. 431).

6. This interpretation is consistent, I believe, with the only other statement I could find that could possibly be misinterpreted. In speaking of the ERE, Mises says that "in order to analyze the problems of change in the data and of unevenly and irregularly varying movement, we must confront them with a fictitious state in which both are hypothetically eliminated" (p. 247). Keeping in mind Mises' meaning of *data,* this statement cannot mean, I maintain, that the ERE can be used to explain or "predict" the direction of change or that the ERE is a starting point for the analysis of particular changes unless by *change* one means a change in desires, value judgments, and so on.

7. A critic might claim textual support for the view that Mises did not use the term ERE to refer to an end point. Instead, the critic would say, he used the concepts of the final state of rest and the final price to refer to this. Consider specifically the following quote:

> In dealing with the plain state of rest, we look only at what is going on right now. We restrict our attention to what has happened momentarily and disregard what will happen later. We are dealing only with prices really paid in sales, i.e., with the prices of the immediate past. We do not ask whether or not future prices will equal these prices.
>
> But now we go a step further. We pay attention to factors which are bound to bring about a tendency toward price changes. We try to find out to what goal [!] this tendency must lead before all its driving force is exhausted and a new state of rest emerges. The price corresponding to this future state of rest was called the *natural price* by older economists; nowadays the term *static price* is often used. In order to avoid misleading associations it is more expedient to call it the *final price* and accordingly to speak of the *final state of rest.* (p. 245)

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> The imaginary construction of the final state of rest is marked by paying full regard to change in the temporal succession of events. In this respect it differs from the imaginary construction of the *evenly rotating economy,* which is characterized by the elimination of change in the data and of the time element. . . . The evenly rotating economy is a fictitious system in which the market prices of all goods and services coincide with the final prices. . . . The same market transactions are repeated again and again. . . . The plain state of rest is disarranged again and again, but it is instantly reestablished at the previous level. . . . [In constructing the evenly rotating economy], [w]e are free to assume that infants are born, grow old, and finally die, provided that total population figures and the number of people in every age group remain equal. Then the demand for commodities whose consumption is limited to certain age groups does not alter, although the individuals from whom it originates are not the same. (pp. 246–47)

The suggestion here is that the ERE is what might be called an "actionless economy." It appears to be a model of an economy in which there is no action, only repetitive behavior. "Automatic action is substituted for the conscious striving of thinking man after the removal of uneasiness" (p. 249).

In this preliminary discussion of the usefulness of the ERE (pp. 246–50), Mises does not indicate that he intends to use the ERE as an end point. Here he suggests only that by contrasting an image of a rigid economy such as the ERE with our knowledge of economic interaction in everyday life, "we are led to the insight that dealing with the uncertain conditions of the unknown future . . . is inherent in every action" (p. 250). This means that we are led to the insight that entrepreneurship exists in everyday life.

Following these preliminary statements, however, it is clear that Mises proceeds to use the ERE as an end point. Consider the following discussion of the prices of factors:

> The tasks incumbent upon the theory of prices of factors of production are to be solved by the same methods which are employed for treatment of the prices of consumers' goods. We conceive the operation of the market of consumers' goods in a twofold way. We think on the one hand of a state of affairs which leads to acts of exchange: the situation is such that the uneasiness of various individuals can be removed to some extent because various people value the same goods in a different way. On the other hand, we think of a situation in which no further acts of exchange can happen because no actor expects any further improvement of his satisfaction by further acts of exchange. We proceed in the same way in comprehending the formation of the prices of factors of production. The operation of the market is actuated and kept in motion by the exertion of the promoting entrepreneurs, eager to profit from differences in the market prices of the factors of production and the expected prices of the products. The operation of this market would stop if a situation were ever to emerge in which the sum of the prices of the complementary factors of production—but for interest—equaled the prices of the products and nobody believed that further price changes were to be expected. Thus we have described the process adequately and completely by pointing out, positively, what actuates it and, negatively, what would suspend its motion. The main importance is to be attached to the positive description. The negative description resulting in the imaginary constructions of the *final price* and the *evenly rotating economy* is merely auxiliary. For the task is not the treatment of imaginary concepts, which never appear in life and action, but the treatment of the market prices at which the goods of higher orders are really bought and sold. (p. 334) (emphasis added)

Mises goes on to attribute the method to Gossen, Carl Menger, and Böhm-Bawerk.

Also consider the following discussion of changes that are initiated by an increase in saving.

> All these changes in the prices of factors of production begin immediately with the initiation of the entrepreneurial actions designed to adjust the processes of production

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> to the new state of affairs. In dealing with this problem as with the other problems of changes in the market data, we must guard ourselves against the popular fallacy of drawing a sharp line between short-run and long-run effects. What happens in the short-run is precisely the first stages of the chain of successive transformations which tend to bring about the long-run effects. The long-run effect is in our case the disappearance of entrepreneurial profits and losses. The short-run effects are the preliminary stages of this process of elimination which *finally, if not interrupted by a further change in the data, would result in the emergence of the evenly rotating economy.* (p. 296) (emphasis added)

Finally, Mises' approach should be evident from the introductory statement to his section entitled "The State of Rest and the Evenly Rotating Economy":

> The only method of dealing with the problem of action is to conceive that action ultimately aims at bringing about a state of affairs in which there is no longer any further action, whether because all uneasiness has been removed or because any further removal of felt uneasiness is out of the question. Action thus tends toward a state of rest, or absence of action. (p. 244)

8. Anyone who doubts that Mises conceived of entrepreneurship so broadly should consider Mises' discussion of the promoter. In this discussion, he begins by distinguishing the entrepreneur from the capitalist-landowner and the worker. He says that the purpose of this is to identify a functional distribution. In this division of functions, the "[e]ntrepreneur means acting man in regard to the changes occurring in the data of the market" (p. 254). Then he goes on.

> Economics, however, always did use the term *entrepreneur* in a sense other than that attached to it in the imaginary construction of the functional distribution. It also calls entrepreneurs those who are especially eager to profit from adjusting production to the expected changes in conditions, those who have more initiative, more venturesomeness, and a quicker eye than the crowd, the pushing and promoting pioneers of economic improvement. This notion is narrower [!] than the concept of an entrepreneur as used in the construction of the functional distribution; it does not include many instances which the latter includes. (pp. 254–55)

9. The term *foil* is apparently taken from Hayek (1941). Hayek states that "it is only by contrast with this imaginary state, which serves as a kind of foil, that we are able to predict what will happen if entrepreneurs attempt to carry out any given set of plans" (p. 23). Interestingly, Hayek also stresses the idea, repeated by C-F and attributed to Mises, that "[t]he ERE can be used to explain or 'predict' the direction of change" (p. 866). This is not the place to describe what I believe to be a substantial difference between Hayek's belief about the usefulness of equilibrium and Mises' belief.

10. Mises says that we can only discover the conditions of "a living world in which there is action" (which means the kind of economic reality that contains a variety of entrepreneurial activities) "by the *argumentum a contrario* provided by the image of a rigid economy" (p. 250).

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Subjective Cost Revisited

William Barnett II

In volume 1 of *The Review of Austrian Economics,* Professor Leland Yeager exhibits a lack of understanding of the concept of (subjective) cost as explicated by Professor Buchanan.[1] Yeager presents the following example:

> Suppose the best course of action open to me is, in my judgment, to open a restaurant of a quite specific type in a specific location. The next best course, then, is presumably to open a restaurant identical in all but some trivial detail, such as the particular hue of green of the lampshades. If so, the cost of the precise restaurant chosen is presumably an all but identical restaurant worth to me, in my judgment, almost fully as much. Generalizing, the cost of a chosen thing or course of action is very nearly the full value that the decisionmaker attributes to it.[2]

This leads him to "a serious question. How far from identical to the chosen course of action must the next best alternative be to count as a distinct alternative?"[3]

It is important to note that Professor Yeager's example does not concern a decision facing some other person, but rather a decision facing himself. Therefore, when he says, "The next best course, then, is presumably . . ." and, "the cost . . . is presumably . . ." in referring to an alternative, we may, and I do, assume that he is relating this information qua decisionmaker and that it reflects the true state of mind of the decisionmaker. Of course, no one but the individual decisionmaker can know what the decisionmaker considered to be the best alternative and the utility he attached thereto, at the moment of decision, except the decisionmaker himself.

Dr. Block, in volume 2 of *The Review of Austrian Economics,* responds: "The only answer consistent with Austrian subjectivism . . . is that it is up to the individual evaluator."[4] This response is inadequate. Block is drawn into it by falling into the old trap of allowing the other person to frame the issue.

I am grateful to an anonymous referee for helpful comments.

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The way in which Yeager formulates the question is misleading; it implies an incorrect understanding of the concept of alternatives. That is, to the extent that the decisionmaker perceives the courses of action to be identical, they are not alternatives; rather, it is only to the extent that the person perceives the courses of action to differ that they are alternatives. Thus choice and, necessarily, cost relate to the differences among possible courses of action, not to the elements that are identical.

Therefore, in Yeager's example, since the difference between the relevant courses of action extends only to the color of the lampshades, the choice extends only to the color of the lampshades. And since the choice extends only to the color of the lampshades, the only cost involved is that of the chosen color of lampshades. This cost is in terms of forgone utility. That is, so as, at the moment of choice, the benefit of the choice resides in the utility he expects to gain from having lampshades of the color of choice, so also the cost of the choice resides in the utility he expects to forgo from not having lampshades of the color he thinks he would choose if he did not choose the color he did choose. Thus the choice was misstated. It was not between opening "a restaurant of a quite specific type in a specific location" and opening "a restaurant identical in all but some trivial detail."[5] Rather, the choice was actually with respect to the trivial detail.

Thus Yeager's question as to the magnitude of difference necessary to make alternatives distinct is seen to be based on a misunderstanding of the concept of alternatives and, necessarily therefore, of the concepts of choice and cost. The correct response to Yeager's question is that it is only the differences among possible courses of action that comprise grist for the mill of choice. Thus choice and, necessarily, cost exist only with respect to these differences. And Yeager's generalization is, of course, false.

Notes

1. James M. Buchanan. *Cost and Choice.* Chicago: Markham, 1969.
2. Leland B. Yeager. "Why Subjectivism?" *The Review of Austrian Economics,* vol. 1 (1987), p. 24.
3. Ibid., p. 25.
4. Walter Block. "On Yeager's 'Why Subjectivism?' " *The Review of Austrian Economics,* vol. 2 (1987), p. 204.
5. Yeager, p. 24.

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Reply to Comment by William Barnett II

Leland B. Yeager

My lampshade example adequately described the supposed situation. Whether the choice "actually" (to quote Professor Barnett) lay between alternative lampshades or alternative restaurants is a purely verbal question to which either answer would be as empty and uninteresting as the question itself. In pressing that question, Professor Barnett shows he did not grasp the purpose of my example, which was to heap deserved ridicule on certain sorts of brooding over the concept of cost.

Ever since being exposed (probably back in college) to the notion of cost as the forgone next-best alternative, I had been vaguely uneasy about it. Only when working on my article in *Review of Austrian Economics,* volume 1, and on my reply to Walter Block in volume 2 did I begin to articulate for myself just what about that notion made me uneasy.

To focus on the decisionmaker agonizing over his choices trivializes the analysis of cost. *Of course* no mere observer can fully enter into the decisionmaker's mind and soul and share his feelings about the merits of the course of action that he finally identifies as next best and accordingly rejects. All this is familiar material and need not be erected into a fundamental insight of economics.

To ramble on about the ineffabilities of choice obscures what is not so obvious and what can be understood only through technical economic analysis. This is the social significance of money cost and the way money cost conveys abbreviated information to the decisionmaker about circumstances in parts of the economic system outside his immediate ken, including even information about subjective circumstances, such as the abilities and preferences of myriad persons whom the individual decisionmaker could not possibly know. The prospective money costs (as well as prospective money revenues) of alternative lines and scales of production—and, more generally, the money numbers associated with alternative courses of action—do not exhaust what a decisionmaker needs to know, but they are an important part. Economic analysis has the task of explaining what those numbers signify in the economywide context, including their role in conveying information and incentives. (Students of F.A. Hayek's

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writings will know what I am alluding to. What I am saying here is compressed, by the way, and should be read along with the qualifications mentioned in my original article.)

The way Professor Barnett latches onto one short paragraph in my article again illustrates a curious trait found in some strands of otherwise admirable writings. This is a tendency to be diverted from substantive economics into pointless profundities of methodology and into brooding over merely verbal issues.

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Comment on Tullock's "Why Austrians Are Wrong About Depressions"

Joseph T. Salerno

Let me preface my comment with the following caveat: I am skeptical of the value of a scholarly journal article that attempts to critically evaluate the "canonical version" of an economic theory, particularly when the theory in question deals with a phenomenon as complex as the business cycle. Added to this is my uneasiness over the fact that the version that is chosen for criticism (Rothbard [1969]) was intended as a popular exposition of the theory. This hardly does justice to the profundity of the Austrian theory of the business cycle or to the scholarship of Murray N. Rothbard. If one wishes to pen a brief critique of the general thrust of Austrian cycle theory, it is more appropriately done as an explicit book review, say, of an anthology such as Mises et al. (1983). Having expressed these reservations, I proceed with my comment.

The three nits that Tullock picks at the beginning of his article in *The Review of Austrian Economics,* volume 2 (pp. 73–74), deserve comment because they bear out the concerns I mentioned in the preceding paragraph. First, the author correctly notes that, in the particular pamphlet under review, "Rothbard never explains why the inflation which is part of his theory cannot simply be continued or even accelerated." But, of course, this question is dealt with in many advanced expositions of Austrian cycle theory. As one of numerous examples, Rothbard (1970, volume 2, pp. 875–77), himself, addresses the issue under the heading of "The Ultimate Limit: The Runaway Boom." Moreover, Tullock's personal testimony that hyperinflation "is undeniably unpleasant, but not really a disaster" (p. 73), while certainly provocative, is irrelevant with respect to this issue. It is sufficient that the political and monetary authorities who orchestrate the inflationary boom fear the eventuality of hyperinflation and act to prevent it. Thus, for instance, the proximate cause of the 1980–82 U.S. depression was the well-publicized decision of the Volcker Fed to "disinflate" the economy from highly unpopular double-digit inflation levels by reining in the growth of money and bank credit.

The author's second nit (p. 73) concerns Rothbard's alleged failure to come to grips with the question of why entrepreneurs do not eventually learn about,

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correctly forecast, and adjust their investment activities to the business cycle. In current jargon, the author is questioning why Austrian cycle theorists do not assume that market participants are capable of formulating "rational expectations," which incorporate a correct theory of economic relationships and preclude systematic forecasting errors. Without attempting to provide an answer to this question here, suffice it to say that the issue has been discussed by a number of Austrian cycle theorists, including Mises (1943), O'Driscoll (1977, pp. 106–08; 1979, pp. 166–68), and Garrison (1986, pp. 445–47). Once again, the author's decision to avoid grappling with the extensive literature on the theory has led him to suggest a lacuna in the theory that simply does not exist.

The final nit Tullock picked out (p. 74) stems from his apparent misunderstanding of the methodological context of the Austrian business-cycle theory. Thus the author faults Rothbard for ignoring the results of statistical tests that suggest that depressions and booms do not follow a cycle but, instead, follow a so-called "random walk." This is beside the point, however, since Austrians do not construe the term *business cycle* as a mechanistic or statistical regularity that openly manifests itself in history, but as a recurring qualitative sequence of abstract economic phenomena that can only be detected in the historical data by the application of theory. In an early contribution, Mises (1978, p. 117) wrote: "Neither the connection between boom and bust nor the cyclical change of business conditions is a fact that can be established independent of theory. Only theory, business cycle theory, permits us to detect the wavy outline of a cycle in the tangled confusion of events." The author could have found a concise and lucid discussion of the methodological foundations of Austrian cycle theory in Rothbard (1975, pp. 1–7).

With regard to Tullock's "major objection" to the theory, his argument (pp. 3–10) is likewise marred by an apparent unfamiliarity with advanced expositions of the theory. I shall not attempt here to give a point-by-point critique of the author's main argument that, during a typical Austrian business cycle, "there would be only minor transitional unemployment [and] measured GNP would be higher as a result" (p. 74). It is enough to point out that the author's conclusion rests on basic misconceptions about Austrian capital theory and structure-of-production analysis.

First, the author appears to ignore the important notion of intertemporal complementarity in the structure of production. Thus, even if the higher-stage investment projects and production processes induced by the artificially depressed interest rate are eventually completed in the technological sense, they still may be underutilized or wholly abandoned during the depression-adjustment phase. The reason is that the products yielded by these higher-order processes confront greatly contracted market demands, resulting from the suddenly revealed increased scarcity (and hence money costs) of the temporally "nonspecific" inputs with which they must be combined in lower-order production processes.

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For example, a newly completed iron ore mine may be abandoned because, at any technically feasible rate of output, the price of the ore has fallen below the "marginal costs" of the mine's operation, including wage rates, prices of fuels, and the rents of power generators and hauling vehicles. Higher prices for the services of labor and of the other relatively nonspecific inputs or "convertible" capital goods are due, in turn, to the fact that too great a proportion of the available stock of these resources was erroneously invested in the production of "inconvertible" or "specific" higher-order goods, such as the iron mine shaft and related "fixed" investments. The higher monetary costs of nonspecific resources, which make their continued employment in certain higher-stage processes uneconomic, simply reflect the fact that such resources have higher marginal revenue products in the lower-stage processes from which they were originally diverted during the inflationary boom. The bankruptcies and resource unemployment occurring in the mining and mining-equipment industries during the depression-adjustment phase are thus part and parcel of the process by which labor and other nonspecific factor inputs are reallocated to finished-goods production and to the wholesale and retail industries. It is the metaphorical "structure of production" itself—not necessarily particular factories or other construction—that cannot be completed, due to the unanticipated scarcity of capital that is suddenly revealed during the depression-adjustment phase.

A second basic confusion of the author involves his apparent belief that Austrian cycle theory indicates that an interest rate temporarily lowered by monetary inflation will lead to general overinvestment in capital *and* consumer-goods industries (Tullock, pp. 5–7). But the main insight of Austrian cycle theory is that the inflationary boom induces "malinvestment," which denotes a diversion of scarce factors and money capital away from consumer-goods industries into capital-goods or, more generally, "higher-stage" industries, including, for example, investments in specially designed computers and software for specific R&D projects, expanding facilities supplying wildcat oil drillers, site planning for new hydroelectric plants, and so on. With scarce resources thus reallocated higher up the ladder of the structure of production, there necessarily occurs at least a temporary reduction in the quantities of final consumer goods produced.

Moreover, the uneconomic commitment of labor services and other nonspecific resources to the expansion of the production of relatively inconvertible higher-stage goods such as industrial construction and equipment will ultimately be revealed in an unforeseen bidding up of wage rates initiated in the lower stages, when it is discovered that available stocks of labor inputs are insufficient to complement the full array of products beginning to flow forth from the overbuilt higher stages. Such intertemporal price variations result in a shifting of labor as well as convertible capital goods into the relatively undermanned and underequipped lower-stage industries and account for the corresponding

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bankruptcies and retrenchments of overcapitalized higher-stage firms, thus bringing about the abandonment of many of the investments—whether technologically completed or not—in inconvertible higher-order capital goods. Even where the latter constitute completely sunk costs, they still may be entirely abandoned, because their continued utilization at any level of output does not generate an income sufficient to cover the "opportunity costs" of their complementary nonspecific factors, such as labor.

It is precisely the abandoned or underutilized factories, equipment, power-generating sites, mines, and R&D projects that represent the "malinvested" capital of the boom period characterized by artificially lowered interest rates. In view of this wasted capital investment, the aggregate capital/labor ratio for the economy and, therefore, marginal productivity of labor and real wage rates can be expected to be lower than if investment of scarce productive resources and the "length" of the production structure had been determined by genuine market time preferences, which are reflected in the unmanipulated or "natural" interest rate. Thus, contrary to Tullock's contentions (pp. 3, 10), Austrian cycle theory *does* explain the observed drop in "measured GNP" and in laborers' living standards during the depression.

At the end of his article (pp. 8–9), Tullock rehearses his earlier objection regarding the explanatory power of the Austrian theory when confronted with rational expectations. This is an important and timely issue and the author could have provided a valuable service by formulating his objection in a manner that speaks to what Austrian theorists have already written on this subject. (The relevant contributions are cited in the third paragraph of this comment.) Having chosen not to do this, however, the author's discussion fails to provoke any new or interesting thoughts on the matter.

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Reply to Comment by Joseph T. Salerno

Gordon Tullock

The outstanding characteristic of the comment on which I comment is that we agree on the basic issue. My point was that the mechanism described by the Austrians would lead, not to unemployment, but to a shortage of labor and higher wages than would be justified without all of this additional capital in the market.

Professor Salerno refers to "higher prices for the services of labor and of the other relatively nonspecific inputs or 'convertible' capital goods." This is in complete accord with my view that the Austrian theory would tighten the labor market. It would increase bankruptcies and suicides by brokers, occasionally lead to incomplete factories, and cause a certain amount of transitional unemployment, particularly in the capital-goods industries, but it could not create the kind of massive unemployment we saw in 1929–33, 1937, or the only depression that he mentions, 1980–82.[1] Massive unemployment is what people mainly complain about with respect to depressions and, indeed, is usually regarded as the principle indicator of the depression. Ignoring it seems odd.

Apparently the Austrians use the word *depression* for something other than what ordinary people use it for. My critic says the business cycle is "a recurring qualitative sequence of abstract economic phenomena that can only be detected in the historical data by the application of theory." Thus, the word *depression* when used by the Austrians has a unique meaning. There is no reason why, of course, the Austrians should not use a special definition of a word, but they really should warn us.

Professor Salerno seems to be denouncing me for employing the word *depression* in the ordinary meaning rather than in the specialized Austrian usage. I regret to say that I must use it in this form because, to put it bluntly, I do not know the specialized Austrian meaning. The standard meaning of the word *depression* is a situation in which general conditions are very bad and, most importantly, there is very high unemployment.

Possibly we can compose our differences by agreeing that the Austrian theory of depressions does not help understanding what the ordinary citizen thinks of as depressions. It explains the Austrian-style "depression," which is

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(as Salerno quotes Mises) a "wavy outline of a cycle in the tangled confusion of events."

But, judging by Salerno's article, apparently there is at least one case in which the Austrian "depression" and the depression in its usual meaning coincide: the 1980–82 U.S. depression. Salerno and I agree that it was caused proximately by the Volcker Fed disinflating the economy.[2] Both of us would also agree that the previous inflation motivated Volcker to disinflate. I do not know whether he would agree with me that President Carter probably would have had a better chance of winning the 1980 election if this decision had not been made. Of course, not having had a previous inflation would also have been politically helpful.

I think that the decision to stop the inflation was politically rather than economically motivated. That seems to be my commentator's view also. Salerno refers to "highly unpopular double-digit inflation levels." There was no economic crisis in 1979 that required immediate action. Indeed, many countries have for long periods of time maintained rates of inflation that high or higher. Italy, of course, is a flourishing economy that has been operating with inflation rates like the United States experienced during the Carter years for quite a long time now.

Apparently Professor Salerno somehow feels that the malinvestment, after first increasing the demand for labor, puts the society in a situation where so much labor cannot be employed. This, in turn, seems to depend on a rather odd pattern of investment during the boom. There would not be more McDonald's going up. Instead there would be more research and capital investment in plants for making McDonald's eventually. Surely this is false. If the interest rate has been forced down and businessmen make the Austrian mistake of not realizing this is temporary,[3] the rate of growth of the capital-producing industry presumably would be higher than the rate of growth of the fixed-capital–using industries like McDonald's. Both should, however, grow. To quote a phrase much used in another connection: "A rising tide lifts all boats." The ratio of capital to labor in the consumer-goods industries would be higher.

In the original article, I used the analogy of a government that used tax money to subsidize investment and then suddenly stopped. I think this policy would be unwise. I do not think it would cause more than a little transitional unemployment, particularly in the capital-goods industry. It would be hard on the capitalist who had half-completed factories, but the additional capital would actually raise wages.

The Austrians and I agree on two things: neither of us likes inflation and neither likes depressions. Indeed, for the 1980–82 depression, both of us are monetarists and believe lowering the derivative of the money supply was the proximate cause. Historically, the lowering of the derivative of the money supply has not infrequently occurred as stabilization after a period of inflation. It has, however, on occasion occurred without such inflation and on other occasions after inflation that was not caused by the government. It should be kept in mind

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that history's first inflation occurred during the reign of Alexander the Great and was a gold inflation.[4]

The basic flaw in the Austrian's line of reasoning as I see it is the desire to argue that the inflation that they object to is the actual cause of the subsequent contraction. In a way, it becomes a moral tale. The wickedness of inflation carries a punishment, albeit the punishment falls more heavily on private citizens than on the government that caused it.

I would not deny that inflation is "wicked," but its main costs are the reductions in efficiency of the economy while the inflation is going on. It is possible to get out of an inflation without a depression. In fact, the more severe the inflation, the easier. It is also possible to have the reduction in the first derivative of the money supply without a preexisting inflation. It can go from zero to a negative number. Indeed, I would say that the 1929–33 experience was an example although I know that Rothbard argues that the 1920s was an inflationary period.

Depressions have been caused by many things. The termination of an inflation can cause a depression, but contraction of the money supply after a period of stability is at least equally dangerous. Frequently there are sins, either of omission or commission by governments, that either cause or aggravate depressions. Inflation is a "sin" that in the present-day world is normally the fault of government. It is inefficient, but its only connection with depressions is that incompetent governments may cause a depression while ineptly trying to stop it.

Notes

1. I follow the usual convention of using the term *depression* for only the down side of the trough.

2. This does not prove that Volcker is a monetarist.

3. Salerno does not answer this particular nit, but simply suggests I do wide reading.

4. Alexander spent the Persian emperor's gold reserve.

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Comment on Professor Timberlake's Squared Rule for the Equilibrium Value for the Marginal Utility of Money

William Barnett II

In both volume 1[1] and in volume 2[2] of *The Review of Austrian Economics*, Professor Timberlake states his "squared" rule for the equilibrium value of the marginal utility of money, to wit:

$$MU_m = \frac{MU_c}{P_c^2},$$

where:

MU_m is the marginal utility of money,

MU_c is the marginal utility of the "composite good"[3], and

P_c is the price of the composite good.[4]

This rule is based on the neoclassical theory of the consumer. Now, even if we ignore the insurmountable analytical problems concerning the concepts of the marginal utility of a composite good, and accept the analysis on its own terms (i.e., using the neoclassical analytical apparatuses of comparative statistics/equilibrium analysis), Professor Timberlake's rule is erroneous. That is, even if the concepts of a composite good and its marginal utility are accepted, his formulation remains a faulty application of neoclassical comparative statics/equilibrium analysis. Thus I shall deal with it on its own terms.

According to the neoclassical theory of the consumer, each consumer is assumed to maximize a utility function subject to a budget constraint. (Professor Timberlake simplifies matters somewhat by considering a "typical individual."[5] Thus:

I am grateful to two anonymous referees for helpful comments.

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maximize: $U[X_i]$ for $i = 1, \ldots n$,

subject to: $Y_i = \Sigma P_i \cdot X_i$,

where:

$U(X_i)$ is the typical individual's utility function,

X_i is the quantity of the i-th good purchased by the typical individual,

P_i is the price of the i-th good,

Y_i is the wealth in monetary terms of the typical individual, and

n is the number of goods.

Assuming the utility function exhibits the necessary mathematical conditions in terms of continuity, differentiability, and so on, the first-order conditions for maximization are:

$$\frac{MU_i}{P_i} = L \text{ for } i = 1 \ldots n, \text{ and}$$

$$Y = \Sigma P_i \cdot X_i,$$

where:

MU_i is the typical consumer's marginal utility of the i-th good and

L is a Lagrange multiplier.

From the first-order conditions, the following can be derived:

$$\frac{MU_1}{P_1} = \frac{MU_2}{P_2} = \ldots = \frac{MU_n}{P_n},$$

or, in more useful form:

$$\frac{MU_i}{MU_j} = \frac{P_i}{P_j}, \text{ for } i = 1 \ldots n, \text{ and } i \neq j.$$

That is, the (subjective) rate at which the typical individual is willing to substitute between any two goods (given by the ratio of that individual's marginal utilities of the goods) must be equal to the (objective to him) rate at which other people are offering to exchange the same two goods (given by the ratio of the prices of the goods).

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In his analysis, Professor Timberlake assumes (implicity) that the typical consumer's utility function and budget constraint may be represented as including but two goods (money and a composite good):

$$U = U(C,M) \text{ and}$$

$$Y = P_c \cdot C + P_m \cdot M$$

where:

C is the quantity of the composite good purchased by the typical consumer, and

M is quantity of money purchased by the typical consumer.

Thus he (implicitly) derives the maximizing conditions as:

$$\frac{MU_c}{P_c} = L$$

$$\frac{MU_m}{P_m} = L$$

$$Y = P_c \cdot C + P_m \cdot M,$$

from which he (implicitly) derives his equilibrium condition:

$$\frac{MU_c}{P_c} = \frac{MU_m}{P_m}$$

This is a legitimate neoclassical formulation provided that P_m is the nominal price of money, that is, the price of money in terms of itself, to wit: one (1). This condition is essential if the budget constraint is to be consistent. Thus, where:

Y is in terms of dollars ($),

P_c is in terms of dollars per unit of the composite good ($/c),

C is in units of the composite good (c), and

M is in dollars ($),

$$\$ = \$/c \cdot c + P_m \cdot \$ \text{ or } \$ = \$ + P_m \cdot \$.$$

It is immediately obvious that for the budget constraint to be consistent, the price of money must be dimensionless, specifically $P_m = 1$. Thus, substituting one (1) for the price of money in our budget constraint yields

$$\$ = \$ + 1 \cdot \$ \text{ or } \$ = \$ + \$,$$

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in which case the dimensions of the budget constraint are consistent.

Note that Professor Timberlake's use of the reciprocal of the price of the composite good $[1/P_c]$ as the price of money, that is

$$P_m = 1/P_c = 1/(\$/c) = c/\$,$$

results in the budget constraint having the following dimensions:

$$Y = P_c \cdot C + P_m \cdot M,$$

$$\$ = \$/c \cdot c + c/\$ \cdot \$ \text{ or } \$ = \$ + c,$$

which obviously are inconsistent.[6]

Unfortunately, Professor Timberlake missed this crucial point. This failure resulted in his substitution of the reciprocal of the price of the composite good for the price of money in his equilibrium condition, yielding:

$$\frac{MU_c}{P_c} = \frac{MU_m}{1/P_c} \text{ or } \frac{MU_c}{P_c^2} = MU_m;$$

or, in more useful form,

$$\frac{MU_c}{MU_m} = P_c^2.$$

In this latter form, Professor Timberlake's error and its source are clear. The rate at which money and the composite good may be exchanged is given by the money price of the composite good, not by its square. It was the improper substitution of "the" real price of money (the reciprocal of the price of the composite good) for the nominal price of money in the budget constraint that caused the squared rule.

We see then that even within the framework of neo-classical analysis, Professor Timberlake's rule is untenable. In addition, from an Austrian perspective, there is an even more fundamental problem with the neoclassicists' attempted mathematization, via the use of the infinitesimal calculus, of economic analysis. In this case, it appears in the form of the unwarranted and implicit assumptions that the composite good be infinitely divisible, that the law of diminishing marginal utility not hold with respect to subdivisions (new units?) of the last unit of the composite good, and that marginal utilities be infinitely divisible, whatever that may mean.

Consider a more appropriate formulation of the neoclassical theory. All notation is as previously given. However, the utility function and budget constraint are expressed in terms of "real variables"—that is, "real income" and "real (money) balances." Real balances is, of course, the name given to the

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purchasing power of actual ("nominal") money balances. Since there is only one nonmoney good (the composite good), both real income and real balances must be, and are, in terms of units of the composite good. That is, their dimensions are:

$$M/P_c = \$/(\$/c) = c \text{ and}$$
$$y = c$$

where

$$M/P_c = \text{real balances and}$$
$$y = \text{real income.}$$

Thus, the utility function and budget constraint are:

$$U = U(C, M/P_c) \text{ and}$$
$$y = M/P_c + C.$$

Note that the dimensions of the budget constraint are consistent.

The first-order conditions for a maximum are:

$$MU_C = L,$$
$$MU\ (M/P_c) = L, \text{ and}$$
$$y = M/P_c + C.$$

Thus

$$MU_C = MU\ (M/P_c).$$

That is, the marginal utility of the last "unit of real balances" purchased by the consumer must be equal to the marginal utility of the last unit of the composite good purchased by the consumer.

But, the problem is that units of real balances are multiples (or possibly fractions) of units of actual money balances. Thus, to use Professor Timberlake's example of a tripling of the money stock, assume the following:

1. The initial stock of money is \$100 ($M = \$100$),
2. The initial stock of the composite good is 500c ($C = 500c$), and
3. The initial price of the composite good is \$1/c, ($P_c = \$1/c$).

Then

$$y = \$100/(\$1/c) + 500c \text{ or } y = 600c.$$

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Let the money stock triple and, in (assumed) consequence thereof, the price of the composite good do likewise.

Then:

1. Then the new stock of money is \$300,
2. The stock of the composite good is unchanged, and
3. The new price of the composite good is \$3/c.

Note that real income is unchanged:

$$y = \$300/(\$3/c) + 500c \text{ or } y = 600c.$$

Then, in the initial situation the marginal utility of the five-hundredth unit of *C* is equal to the marginal utility of the last unit of real balances, which is the amount of actual money necessary to buy a unit of the composite good. In this situation, the unit of real balances is \$1. That is, the marginal utility of the five-hundredth unit of *C* is equal to the marginal utility of the one-hundredth dollar.

In the subsequent situation, the marginal utility of the five-hundredth unit of *C* is still equal to the marginal utility of the last unit of real balances. However, in this case, the unit of real balances is \$3. That is, the marginal utility of the five-hundredth unit of *C* is equal to the marginal utility of the two-hundred-ninety-eighth through the three-hundredth dollars, *taken as a unit.* Unless the composite good is infinitely divisible, it is meaningless to speak of the marginal utility of the last unit of actual money, i.e., the three-hundredth dollar, for it will only fetch one-third of a unit of *C*, if such may be purchased. And, even if this is possible, the marginal utility of the three-hundredth dollar would be equal to the marginal utility of the "last third" of the five-hundredth unit of *C*, and not to one-third of the marginal utility of the five-hundredth unit of *C*. That is:

$$\text{Initial situation: MU (100th \$)} = \text{MU (500th C)};$$

$$\text{Subsequent situation: MU (298th, 299th, and 300th \$s, as a unit)} = \text{MU (500th C)}.$$

However, this does not mean that

$$\text{MU (300th \$)} = 1/3 \cdot \text{MU (500th C) but, rather that}$$

$$\text{MU (300th \$)} = \text{MU (last 1/3 of 500th C)}.$$

Thus, for Professor Timberlake's analysis to be correct, these latter two equations must be identical. That is:

$$\text{MU (last 1/3 of 500th C)} = 1/3 \cdot \text{MU (500th C)}.$$

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And this must hold true for any possible increase or decease in the money stock and attendant price change. But, as mentioned already, this requires that the composite good be infinitely divisible, that the law of diminishing marginal utility not hold with respect to subdivisions (new units?) of the last unit of the composite good, and that marginal utilities be infinitely divisible.

The foregoing analysis demonstrates some of the errors in the analysis leading to the squared rule. These errors are, unfortunately, typical manifestations of what is perhaps the most serious problem in monetary/business cycle/macroeconomic analysis. This problem is inappropriate aggregation, which arises out of attempts to make economic analysis mathematically tractable and to make economic data grist for the mill of statistical (i.e., econometric) analysis.

Notes

1. Richard H. Timberlake, Jr., "A Critique of Monetarist and Austrian Doctrines on the Utility and Value of Money," *The Review of Austrian Economics*, vol. 1 (1987), pp. 93–94.

2. Richard H. Timberlake, Jr., "Reply to Comment by Murray N. Rothbard," *The Review of Austrian Economics*, vol. 2 (1987), pp. 189–97.

3. Of course, except in the imaginations of some people, including many famous economists, "composite good(s)" do not exist. The attempt to create mathematically tractable models of the economy has led many to accept such fiction(s) as reasonable approximations to reality. Unfortunately, this has resulted in much confusion and misunderstanding, and in fact to the foisting of much relatively low level mathematical exercises and games as advanced theoretical economics.

4. The system of notation used in this comment is somewhat different from Timberlake's, but the reader should have no trouble reconciling them.

5. *The Review of Austrian Economics*, vol. 1, p. 93.

6. If the editors of journals required economists to assign dimensions to the variables in their mathematical and statistical equations, many errors could be avoided.

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Marginal Utility Equilibrium between Money and Goods: A Reply to Professor Barnett's Criticism

Richard H. Timberlake, Jr.

William Barnett has offered what he purports to be a criticism to an appendical note that accompanied my article on the marginal utility and value of money in the previous volume of *The Review of Austrian Economics*.[1] Barnett's conclusion to the first section of his comment is that the equilibrium condition between marginal utilities of goods and money relative to their prices is a "legitimate neoclassical formulation *provided that . . . the nominal price of money, i.e., the price of money in terms of itself,* [is] *to wit: one (1).* This condition is essential if the budget constraint is to be consistent." (Emphasis added. I presume that Barnett means by "consistent" "not violated.") He then remarks that my use of the price of money as the reciprocal of the price of the composite good (1/P(c)) is "improper" and an "error."

The fact of the matter is that Barnett's "correction" of my "error" is his reformulation and not an error of mine at all. My economy has in it commodities (R) and their prices (P_R), which specify a monetary value of real goods ($P_r \cdot R$). It also has in it a quantity of nominal money units (M) and a real price for those units of money (pm). The physical goods composing (R) have real value, and so does the nominal money (M) that exchanges them. If an exchange takes place between money and goods, the exchange itself is witness to the fact that the elements in the exchange are part of an equality. Thus, the price in terms of money that one pays for goods must be equal to the price in goods for which the other party to the exchange pays in money.

My inclusion of real money, as the theorists say, "in the budget constraint," was no error. Money in my theory is just as real as goods. If it were not, I would discard it with the rest of the waste paper.

Barnett asserts that the "price of money in terms of itself [is] one (1)," and, again, that money is "dimensionless." This contention for a generalized analysis of money exchanging for all goods in all markets is ridiculous. My question is: one WHAT?

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Let me assert that I *want* real money in my utility function because nominal money by itself is meaningless. The modest analysis I made in my appendical note was to show how changes in nominal money accompanied by corresponding changes in money prices would lead to an adjustment in the marginal utility schedule for money. Within this changing framework, the real stocks of money and goods stayed constant, so the budget constraint was not violated. In Barnett's model, money has no real value, so his equation may be "consistent" in the way he has set it up, but it is also meaningless. Since when does a money-using economy have no real money in it, except during the final gasp of a hyperinflation?

Barnett's "correct" mathematics but flawed economics appears midway through his comment. He states that my budget constraint has the following dimension (I use his notation with brackets added for clarification):

$$Y = [P_c \cdot c] + [P_m \cdot M],$$

$$\$ = [\$/c \cdot c] + [c/\$ \cdot \$], \text{ or}$$

$$\$ = \$ + c.$$

Since \$ cannot equal \$ + C, "my" budget constraint is inconsistent.

Barnett's error here is that he has not included the services of real money-wealth in the original budget constraint. His *Y* is the real income of the composite good *without money*. Real money, it is true, is a real capital stock—wealth; in an "income model," this wealth must be converted into an income flow. In my grammatical model, I had no trouble making such an inclusion. In fact, I discussed this matter at length in my original article, which appeared in volume 1 of *The Review of Austrian Economics*.

Barnett concludes his criticism of my squared relationship with this statement:

> In this latter form, Professor Timberlake's error and its source are clear. The rate [price] at which money and the composite good may be exchanged is given by the money price of the composite good, not by its square. It was the improper substitution of "the" real price of money (the reciprocal of the price of the composite good) for the nominal price of money in the budget constraint that caused the squared rule.

Barnett's observation in the next-to-the-last quoted sentence is a *reductio ad absurdum*. The price of goods in terms of money, believe it or not, is given by the money price of the composite good (!) "and not by its square." My "squared" conclusion for the marginal utility of money, however, does not argue that the *price* of money in terms of goods is squared. It only argues that the *utility* of the last unit of nominal money held in equilibrium will decline as the *square* of the increase in prices, due to the fact that the quantity of money

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units has proportionately increased *and* that the marginal utility *schedule* of all money units has likewise fallen by this same magnitude. (The graphical expression of this change was given in the figure in my appendix.)

In his eagerness to frame my analysis in neoclassical mathematics, Barnett has violated the conditions I set forth in my model. I had economic man in equilibrium with money and goods. Then I promoted a formalized inflation by means of a specified increase in the quantity of nominal money units—the rate of inflation being in proportion to the increase in money. Throughout my analysis, the real quantity of money and its total utility remained statically constant. I therefore aimed at getting economic man into equilibrium with the nominal money units extant, but within the environmental framework of a constant real stock of money.

For Barnett to state that my "substitution of the 'real' price of money . . . for the nominal price of money" was "improper" is incorrect. It may be "improper" if one is mired down in mathematical assumptions, but it is not improper in the real world, and it most certainly is not a "substitution" in my model. Rather, it is a feature I want in the model. Nor does this inclusion violate any budget constraint. My model starts and ends with the same real quantities of money and goods. Why, indeed, would anyone hold any money if the money were only "nominal" as Barnett suggests? My analysis puts real goods and real money into the behavior function of economic man. When an economist states, as Barnett does, that the price of money is "one," or that money is a "mere numeraire" (a common observation), he has abandoned the analysis of money altogether or he has never begun. If holdings of real goods are being analyzed in terms of money, the money must be real as well as nominal.

In the interest of brevity, and because Barnett's criticism of my marginal utility analysis is trivial and confused, I do not treat this latter part of his comment.

Note

1. "Reply to Murray N. Rothbard," *The Review of Austrian Economics*, vol. 2 (1988), pp. 194–97.

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Professor Caldwell on Ludwig von Mises' Methodology

J. Patrick Gunning

> He who addresses fellow men, who wants to inform and convince them, who asks questions, can proceed in this way only because he can appeal to something common to all men—namely, the logical structure of human reason.
>
> —Ludwig von Mises, *Human Action,* p. 35

Bruce Caldwell's recent writings on the Austrian school of economics (1982, chapter 6; 1984) are undoubtedly a positive contribution to economic thought. Caldwell argues correctly that

> The usual criticisms of Austrian economics . . . have thus far proven unsuccessful, primarily because they proceed by assuming the validity of a rival epistemological system (e.g., positivism or falsification), then "proving" that the Austrian system does not meet the qualifications of their presumptively true system. (1982, p. 6)

Or, more specifically, he says:

> (1) Many of the usual complaints against praxeology, so often considered to be conclusive, are anticipated and answered by Mises, and thus must be considered at this point inadequate, and (2) those that remain fail to take into account that the epistemological and methodological foundations of Austrian and neoclassical economics differ. (1982, p. 119)

These arguments carry considerable force since Caldwell is an "outsider" and is well read. He is familiar with both the most significant so-called economic methodologies and the major works in the philosophy of science and social science. Thus, he is like an informed tourist who has ventured into Austrian territory and returned to tell of his journey. But his journey has not been superficial. By focusing on the fundamental and difficult works of Mises, he is able to observe the best of a consistently Austrian position.

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Although Caldwell is sympathetic with what he takes to be Mises' position, he does not go so far as to claim that Mises has been right all along and that other methodologies are wrong. His moderate conclusion is that the door is open for further debate and that such a debate may eventually succeed in resolving some of the contradictions or differences among alternative methodologies (1984, p. 373).

In effect, Caldwell claims that the rivalry between Austrian and alternative methodologies is far from over and he expresses the hope that the outcome of a future debate and cross-fertilization (which will undoubtedly be stimulated by his work) will be greater understanding. Although he keeps an open mind on the subject, his book advocates what he calls methodological pluralism (1982, chapter 13). He does not expect that Austrian methodology will be the "victor."

In both his book and his articles, Caldwell competently shows that Mises anticipated and answered the "external" criticisms of his a priorism. But then, as if to announce that he was able to go beyond Mises, Caldwell offers what he calls internal criticisms—i.e., criticisms that even Austrians (including presumably Mises himself, if he were alive) might accept.

I believe that Caldwell fails to uncover the essence of the Misesian system. As a result, his representation of it, although deeper than that of most of its critics, nevertheless fails to communicate its most fundamental strengths. That Caldwell does not communicate the strengths of Misesian economics is evidenced by the internal criticisms he suggests.

The first section of this article describes Misesian methodology. In the next, three of Caldwell's criticisms are discussed and refuted on the basis of text references. The final section briefly discusses the general problem of comparing Misesian methodology with alternatives.

Misesian Methodology

One cannot successfully evaluate criticisms of "Mises' methodology" without a more complete understanding of *Human Action* than Caldwell provides. It is easy to understand why Caldwell would have chosen not to present the argument in detail. Most economists are not interested in meticulous philosophical argumentation. Unfortunately, without giving the reader an idea of that argumentation, one can neither adequately present Misesian economics nor respond to Caldwell's so-called internal criticisms.[1] Thus, at the expense of taxing the reader's patience, I shall try in this part to summarize the relevant aspects of Misesian methodology.

The Relations in the Logical Structure of the Mind

The foundation of Misesian economic theory is contained in Mises' discussion of the a priori and can be found in *Human Action*. He discusses the a priori

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in a section entitled "The Formal and Aprioristic Character of Praxeology," where he introduces what he calls "the problem of the a priori." This problem, he says, "refers to the essential and necessary character of the logical structure of the human mind" (1966, p. 34). Every attempt to prove the presence of the relations in this logical structure "must presuppose their validity. It is impossible to explain them to a being who would not possess them on his own account" (1966, p. 34). Mises says that all (normal, adult) human beings possess these relations: "Everybody in his daily behavior again and again bears witness to the immutability and universality of the categories of thought and action" (1966, p. 35). Mises invents the term *methodological apriorism* to refer to this set of statements: "The fact that man does not have the creative power to imagine categories at variance with the fundamental logical relations and with the principles of causality and teleology enjoins upon us what may be called *methodological apriorism* (p. 35).

What then are the relations that comprise the "essential and necessary character of the logical structure of the human mind" (p. 34)? Mises derives them from his definition of human action. In his section entitled "The Prerequisites of Human Action," he cites three such prerequisites: (1) man's "eagerness to substitute a more satisfactory state of affairs for a less satisfactory [state]," (2) his imagination of "conditions which suit him better," and (3) "the expectation that purposeful behavior has the power to remove or at least alleviate the felt uneasiness" (pp. 13–14).

It is tempting to refer to these statements as Mises' beliefs. But the word belief carries a relativistic connotation that reflects neither Mises' thoughts nor the philosophical tradition from which they stem.[2] The tradition is fundamentally Cartesian. For Mises, the statement "I am human, therefore I act" carries a degree of forcefulness that is comparable to the statement "I think, therefore I am." One cannot imagine himself being human without implicitly assuming that he acts just as one cannot imagine himself thinking without implicitly assuming that he exists.

Upon reading Mises' description of these prerequisites, an outsider is inclined to think that it is a description of the subjects of his studies—in economics, a description of the economic actors, perhaps with the same status as *homo economicus*. This is not wrong. But it is incomplete. More fundamentally, the description is intended to apply to all normal human beings, including the economist and the comparer of methodologies.[3] Because of this, it can be regarded as an appeal to the intuition of the reader. Mises asks the reader whether he would deny that he possesses the three prerequisites. If the reader honestly denies that he possesses them, Mises would presumably have nothing to say to him. As previously quoted, "It is impossible to explain [the logical relations] to a being who [does] not possess them on his own account." The reader could not be called a human actor.

Mises deduces (defines) the relations in the logical structure of the human mind from the previously named three prerequisites of human action. First,

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he deduces (defines) the idea of *causality.* The human actor must "know" what it means for his action to cause a state of affairs to be different from what it otherwise would be:

> Acting requires and presupposes the category of causality. Only a man who sees the world in the light of causality is fitted to act. In this sense we may say that causality is a category of action. (1966, p. 22)

The concept of causality is not a simple one. In particular, there is the issue of infinite regress. If A is caused by B, what then causes B? One is sometimes inclined to approach this problem by invoking teleology—the idea that there is a first cause. These two ideas, causality and teleology, coexist in the mind, says Mises. He says that both causality and teleology are deducible from the prerequisites of action (pp. 22–25).

Also deducible from the prerequisites of action are the concepts of time and uncertainty. First, consider time.

> The notion of change implies the notion of a temporal sequence. . . . The concepts of change and of time are inseparably linked together. . . . Human reason is . . . incapable of conceiving the ideas of timeless existence and of timeless action. (p. 99)

Now consider uncertainty.

> The uncertainty of the future is already implied in the very notion of action. . . . [Action and uncertainty are] two different modes of establishing one thing. . . . If man knew the future, he would not have to choose and would not act. (p. 105)

Praxeology

Economics, to Mises, means economic theory. Economic theory, in turn, is said to be a branch of what Mises calls *praxeology.* Thus, it is sensible to define praxeology before defining economics. Praxeology is a theory—a set of deductions based upon assumptions. Praxeology provides a framework that is used to help one explain historical events. (Since all known human events are past events, the term historical can be dropped without any loss in meaning.)[4]

Any historical event may be partly the consequence of nonhuman factors, such as the particular physical environment. It may also be partly the consequence of genetic evolution, the constraints of law, and nonpurposeful behavior. From the perspective of praxeology, however, it is most important that historical events are partly the consequence of human action. Because of this, there is a crucial distinction in Mises' conception between observing a historical event and understanding the event.

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To observe a historical event, one merely records "facts" in a way that can presumably be recalled and perhaps communicated to others. To understand a historical event, by Mises' definition, one must identify the human beings whose actions in some way helped to cause the facts that are observed. Then he must insert his hypotheses about their actions (i.e., their ends and means) into a theory of interaction. The important point is that he must have a theory of the interactions of human beings, who possess the a priori categories.

It follows that any comprehensive description of particular historical events must be made partly in terms of a language (or theory) for communicating hypotheses that involve human action (the three prerequisites and the concepts Mises calls causality, teleology, time, and uncertainty). The theory that Mises develops to help describe (i.e., understand) the specifically human character of historical events is praxeology.[5]

Economic Theory

Economic theory, as defined by Mises, is one part, or branch, of praxeology. It is designed specifically to enable one to understand human action conducted on the basis of monetary calculation. Everyday economic action is entangled with action not conducted on the basis of monetary calculation. Moreover, human action itself is entangled with nonpurposeful behavior. The physical environment and law may often lead to nonpurposeful behavior. Accordingly, economic theory, by its nature, does not entail the construction of images of an entire historical event. It commands one's attention to the economic aspects of historical events, i.e., those aspects that are caused by choices made on the basis of monetary calculation.

As theory, economics is a deductive system.[6] Because it is about the distinctly human (i.e., logical or rational) part of human actors, its deductions are made on the basis of the a priori assumptions stated previously. To construct an economic theory, one combines the a priori assumptions with other, *subsidiary* assumptions. Subsidiary assumptions include the assumptions that groups of individuals use money, that they get disutility from labor, that they use capital, and especially that particular types of entrepreneurship are present. Some subsidiary assumptions are made in order to simplify by abstraction, since economic interaction is so complex. An example is the division of individuals into roles, such as the household, the business firm, and the banker.

In economic theory, subsidiary and simplifying assumptions are separate from the a priori "assumptions" that correspond to the a priori categories. To Mises, the a priori assumptions are necessary because they set apart the distinct phenomena of praxeology and because they are undeniable. Economists, as Mises defines them, must use these assumptions. Since economic subjects are human actors, their actions must manifest the assumptions.

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How can one tell whether an assumption is a priori or subsidiary? The answer, according to Mises, is that it is impossible to imagine an alternative to an a priori assumption. For example, with respect to a priori assumptions, Mises says: "[As human beings ourselves, we] cannot think of a world without causality and teleology" (1966, p. 35). Subsidiary assumptions are different: "The disutility of labor is not of a categorial and aprioristic character. We can without contradiction think of a world in which labor does not cause uneasiness, and we can depict the state of affairs prevailing in such a world" (p. 65).

As previously mentioned, the a priori assumptions may give the appearance of being an alternative to *homo economicus*. Thus, it may seem reasonable to compare the "a priori being" with *homo economicus*. To Mises, however, the a priori being is the universal being of which *homo economicus* is one manifestation. The a priori being may, in certain circumstances, act identically to *homo economicus*. But the a priori being also has the option not to act like *homo economicus*. Indeed, all thinkable options must be regarded as being available to the a priori being. *Homo economicus,* however, could never become an a priori being. *Homo economicus* must always maximize his financial wealth in situations specified by the economist. The a priori being can create his own situations. He can even become an economist.

To put this still differently, *homo economicus* is a puppet or robot who is programmed by the economist. The a priori being is his own controller. It is the a priori being who encompasses the humanness in the human being. Economic models must be peopled by individuals who are programmed by the modelbuilder.[7] They cannot be peopled by a priori beings.

The usefulness of models lies in the fact that they enable the modelbuilder to contrast (1) what he can, through reflection, come to know about the nature of the a priori being with (2) the puppet or robot that he employs in his model. In this way, he can isolate the inventive, creative, programming, controlling, alert aspects of human nature. In the context of economics, this means that by constructing models of an economy, he can set apart the characteristics of his a priori beings. He can also learn to attach more realistic characteristics to his roles. He does this by making reference to entrepreneurship and other catallactic functions.[8]

Discussion

It has been shown that Mises defines the human being in terms of a priori categories derived from the three prerequisites of action. He even goes so far as to suggest that persons who do not possess such categories are not normal human beings. It must now be asked whether there is any justification for defining the normal human being as Mises does.

It seems to the author that the possible viewpoints on this matter can be divided into three categories that would be progressively more agreeable with

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Mises. The first viewpoint is that Mises is making a personal judgment about what constitutes humanness. This viewpoint holds that Mises is entitled to his judgment. But there is no reason why another judgment might not be more acceptable. If some other "economist" chooses to define a human being as compassionate, empathetic, status-seeking, driven by a need for companionship, or whatever, his choice is just as legitimate as that of Mises. Similarly, an economics based on the other economist's definition of humanness ought to be every bit as acceptable as one based on the concept of human action, although it may be unfamiliar and novel.

A second viewpoint is a modified version of the first. It is that Mises' concept of human action captures part of what it means to be human, but it does not capture all of it. It should be supplemented by an economic theory that recognizes compassion, empathy, and so on. In this view, Mises' assertion about humanness is at best limited. One should be particularly careful to limit discussions of policy by emphasizing that they only apply to the imaginary world in which the only objective of human beings is to remove uneasiness in ways that can be accomplished by means of markets.

A third viewpoint represents the conviction that Mises was able to isolate a particular set of phenomena that are both important and capable of being analyzed by the methods he suggested. His definition of economics is immanently sensible because it separates this realm from other realms. Deductions in an economic theory so defined cannot yield information about what is best for human beings. But, to the extent that human action (as defined by Mises) is present, the deductions can give one valuable insights about some aspects of everyday interaction. To completely describe interaction, such deductions should be supplemented by what Mises would presumably call nonpraxeological theories. To describe everyday events, one would also have to use knowledge of the natural sciences and knowledge of how coercion is used.

Caldwell's Misinterpretations

Given that the interpretation of Mises presented in this article is correct, it is not difficult to show that the three "internal criticisms" offered by Caldwell were answered by Mises. Each is discussed in turn.

Nonpurposeful Action

The first of Caldwell's suggestions is based on the idea that there can be nonpurposeful action. Caldwell cites the possibility, also suggested by Nozick (1977), that a type of nonpurposeful action is operantly conditioned behavior. Caldwell comments: "An uninformed observer would interpret such behavior as purposeful, but because it is conditioned behavior it is in fact non-volitional

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and hence non-purposeful." Then he says that this may raise a problem for Misesian methodology because Mises "claims" that all human action is purposeful (1984, p. 375).

The answer to this is that Mises does not claim that all human behavior is purposeful. What Mises says is that human action is by definition purposeful. Caldwell confuses behavior with action and a claim with an a priori assumption. Mises says that a theory of human action (praxeology) is a necessary part of any explanation of human behavior. But it is not the only part. A theory of nonpurposeful behavior also is a necessary part of a complete explanation of human behavior. Mises did not conceive of economic theory as a complete theory relating to human behavior. Thus, this particular Nozick criticism is beside the point.[9]

It can be said that Mises claims that purposeful behavior exists and that it is the distinct characteristic of the human being. But the word *claim* seems to greatly understate Mises' view. Mises says that he, the reader, and all normal human beings can recognize the purposefulness of others in the same species by recognizing the purposefulness in themselves. It is difficult to see how such a "claim" could be contradicted.

It is odd that Caldwell considers operantly conditioned behavior a source of possible internal criticism, since he clearly recognizes "that in Mises's system all action is rational because all action is by definition purposeful" (1982, p. 119). The only sensible explanation is that Caldwell failed to realize the difference between a theory of action, as Mises defined it, and a theory of all behavior. It is legitimate to criticize Mises for not being interested in nonrational (nonpurposeful) behavior. One might even argue on ethical grounds that economists should be more concerned with nonrational behavior than with rational behavior. But such a criticism would not be a criticism of Mises' methodology of economics. It would be a criticism of the scope that is encompassed by Mises' definition of economics.[10]

Alternative a Priori Assumptions

Caldwell's second suggestion is that there may be alternative a priori assumptions. This possibility is most extensively pursued in his 1982 book, (pp. 130–33), where he contrasts Mises' methodological apriorism with what he calls the apriorism of Hollis and Nell (1975). Hollis and Nell assert that the "reproduction of the system . . . is primary." If I understand Hollis and Nell correctly, they argue that production and reproduction is necessary because otherwise there would be no "system" to discuss.

It is easy to see that Misesian human action implies production in the sense that action itself means production (choice) of a state that is less unsatisfactory to the actor than an alternative state. The Hollis and Nell assumption does not imply human action, however. It applies equally to nonhuman life

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and to human life. Animals and plants produce and reproduce "systems," but we cannot use our self-understanding and intuition to determine with comparable confidence that they can construct images of the behavior or actions of other animals and plants. In any event, we can be fairly certain that animals and plants do not have methodological thoughts or engage in methodological discussions.

It is possible that Hollis and Nell would separate specifically human "systems" of production and reproduction from nonhuman systems. If so, they may identify a specific category of behavior that would correspond to Mises' concept of action. But there is nothing in their alleged a priori assumption to suggest that they would do this. Thus, one might expect that a theory built on the Hollis and Nell assumption would enable the theorybuilders to capture some aspects of all life. However, there would be no reason to expect such a theory to have anything perceptive to say about economic interaction in the Misesian sense.

If there are indeed a priori assumptions that can be considered as alternatives to those made by Mises, they must apply to the philosopher, the economist, and the subjects studied by the economist. And, if the assumptions are to deal with the same subject matter that Mises was concerned with, they must relate to human thinking, choosing, and planning.

Theory versus Hypothesis

A third suggestion relates to hypothesis testing. Caldwell says that "Mises' view that the predictions which emerge from praxeology cannot and should not be used to test the theory directly conflicts with the standard approach" (1984, p. 371). This statement does not adequately represent the "conflict." Praxeology does not generate predictions. Praxeological and economic models, which are based on a priori assumptions and subsidiary assumptions, yield deductions. If subsidiary and simplifying economic assumptions are realistic, the deductions will represent a "real" economic phenomenon, in Mises' sense. But whether an observer of everyday life can separate that "real" economic phenomenon in Mises' sense from the noneconomic phenomena with which the economic phenomenon is invariably entangled depends upon his knowledge of the noneconomic phenomena.

The conflict is not over whether predictions can or should be tested. One conflict is over whether an economic deduction in Mises' sense is the same as the hypotheses that are tested by "the standard approach." A second conflict is over whether testing an economic deduction can be adequately accomplished when the tester lacks knowledge about noneconomic phenomena and when he cannot directly observe the fundamental economic phenomena.

Thus Caldwell's conclusion that "Surely, one way to discover whether a mistake in reasoning has been made is to see if the chain of logic leads to predictions that are disconfirmed by evidence" (1984, p. 371) seems misplaced and

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confusing. Misesian economic theory, by itself, does not necessarily yield predictions about the phenomena of everyday life. The only way to use Misesian economic theory to generate such predictions is to employ subsidiary assumptions that reflect the "true" preferences, the environmental conditions (i.e., natural science, law, culture, the capital structure, and other resources), and individuals' knowledge of these conditions.

This is not to deny the significance of fact gathering. Rather, it is to put the task of fact gathering in a separate category from the task of constructing theory. Both are necessary if one wishes to describe historical events or to make predictions in everyday life. But the idea of testing the deductions in a model designed specifically to help one identify the characteristics of human beings that have not or cannot be modeled (e.g., entrepreneurship) is quite a bit different from what most economists have in mind when they speak of testing a hypothesis.[11,12]

On the Problem of Comparing Methodologies

In an important sense, Caldwell's work was exploratory. His "internal criticisms" were apparently not meant to denigrate Misesian economics. On the contrary, in his book at least, he issued a "challenge to the Austrians to come up with some means by which their system could be compared with its rivals" (1982, pp. 134–35). Within the context of this challenge, it seems appropriate to devote the final section to the issue of how the various methodologies could be compared.

Caldwell set a difficult task for himself when he sought to compare the "methodology" of Mises with positivism and falsificationism. A crucial part of his comparison should have been to determine whether these methodologies were focused on the same phenomena. Had he done this, I have no doubt that he would have discovered that Mises restricted the definition of economic phenomena to the realm of the purely purposeful and subjective. Mises did not by any means deny the significance of nonpurposeful and objective phenomena. He simply did not regard them as directly relevant to the problem of constructing a pure logic of human action.

Mises presented what, from the perspective of positivism or falsificationism, may be considered a rival definition of the subject matter of economics. In his economic theory, he was not interested in most of the phenomena that modern positivists or falsificationists (i.e., most modern economists) would label economic. His concern was with the choices that cause wants to get satisfied in a capitalist economy. In other words, he was concerned with "entrepreneurial" choices and their consequences and with other factors that individuals regard as instrumental or relevant in the satisfaction of wants.

Positivists or falsificationists are interested at least partly in entrepreneurship and these other factors. But they are also interested in such items as the

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rate of statistical unemployment, the purchasing power of money, the size of the GNP, the magnitude of investment spending, the market interest rate, and other statistical figures.

It is true that these numbers are partly caused by entrepreneurship and that they are related to the other factors. But the numbers also reflect the conditions of the natural environment, prevailing law, and the particular preferences, knowledge, and habits that have been transmitted through cultural processes. Because Mises did not regard these latter conditions as economic, the statistics were not so relevant to him. Since Misesian economics focuses on entrepreneurial choices, the presence of which could not often be discovered by means of statistical analysis, Mises regarded positivism and falsificationism as largely irrelevant to economics.[13]

A proper comparison of Misesian methodology with that of positivism or falsificationism requires the comparer to designate the phenomena with respect to which the methdologies are being compared. Misesian economics is mainly concerned with entrepreneurship and its consequences. Modern positivist or falsificationist economics is defined more broadly; yet it includes entrepreneurship and its consequences. It follows that a proper comparison of the two should be based only on the common phenomena—entrepreneurship and its consequences.

This is the comparison that Mises typically made. Unfortunately in the author's view, Mises' terminology was not suitable to the ideas he wished to convey. As a result, too few readers of *Human Action* recognized that his provocative criticisms of the use of positivism, scientism, statistics, and mathematics were based on his view that economics should be defined differently than it was coming to be defined in the mainstream literature and in the universities. It is hoped that this article will succeed in redirecting the interested reader's attention to the definition of economics that Mises had in mind.[14]

Notes

1. That a more complete presentation is necessary is apparently substantiated by the more recent criticisms of Caldwell that were published in *History of Political Economy*. In the first, Abraham Hirsch mainly criticizes Caldwell for not providing a more detailed justification for his sympathy with Mises. According to Hirsch, Caldwell should have dealt with what most readers, Austrian or not, would regard as contradictions in *Human Action*. Most importantly, says Hirsch, he should have explained how Mises was able to reach such sweeping normative conclusions about capitalism on the basis of his theory of human action, which ignores "nonrational" behavior.

It would not have been necessary for Caldwell to discuss all of *Human Action* to avoid this criticism. He would only have had to recognize that the issue of methodology is sufficiently divorced from that of application that it is possible to elucidate and evaluate the foundation of praxeology, as well as Mises' justifications

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for his methodological apriorism, without referring at all to what Mises thinks or claims such a method implies for the "good of individuals" under capitalism. One can justifiably criticize Mises' own economic reasoning as well as his apparently normative conclusions without ever mentioning or understanding his methodology.

In the second criticism, Eugene Rotwein criticizes Caldwell for not providing sufficient support for methodological apriorism to justify Caldwell's conclusion that Mises adequately defended it against the standard criticisms. Whether Caldwell did or did not provide sufficient support, it is evident that Rotwein himself did not come to understand Mises' arguments either directly by reading Mises' text or indirectly by reading Caldwell. Of particular significance is Rotwein's contention that "the general empirical spirit of science operates to check the growth of dogma, and patently so as compared with apriorism, which is the equivalent of a claim to the possession of a pipeline to the Deity" (p. 671). Only someone who had not come to understand the history of subjectivism and its role in the emergence of Mises' apriorism could reach such a conclusion. It is precisely the fact that "the general empirical spirit of science" (which itself is a manifestation of subjectivism) does not function to check the growth of dogma in the science of human action that compels the scientist to employ a different check—that of subjectivism. However, the employment of subjectivism itself entails the use of a priori assumptions about the nature of subjects and the scientists. The greatest contribution of Mises to social-science methodology was his recognition and elucidation of this.

2. An interesting paper regarding philosophical foundations of Austrian economics has been written by Barry Smith (1986). This should be compared with Mises' *Notes and Recollections* (1978).

3. For the definition of *normal,* see Mises (1966, p. 14).

4. The word *explain* encompasses (1) personal interpretation and (2) communication of one's personal interpretation to others.

5. The word *understand,* as used here, is derived from the German concept of *verstehen.* Mises contrasts it with concept formation. See Mises (1981, chapter 3; 1966, pp. 47–59). Also see Lachmann (1971, chapter 1).

6. *Economics* and *economic theory* are synonymous. The synonym for *applied economics* is the economic understanding of history.

7. Mises calls a model an imaginary construction in order to emphasize the point that a model in economics is not intended to be a small representation of a larger reality, like a scale model of an industrial park.

8. Praxeology employs a priori reasoning (Mises, 1966, p. 32). In discussing a prior reasoning, Mises says:

> The significant task of aprioristic reasoning is on the one hand to bring into relief all that is implied in the categories, concepts, and premises and, on the other hand, to show what they do not imply. It is its vocation to render manifest and obvious what was hidden and unknown before. (1966, p. 38)

Also see Mises (1966, pp. 251–55).

9. For a thorough, point-by-point critique of Nozick's Interpretation of Mises, see W. Block, "On Robert Nozick's 'On Austrian Methodology' " (pp. 397–444).

10. Caldwell also refers in passing to Nozick's view that methodological individualism as practiced by "Austrians" constitutes reductionism. Nozick says: "The methodological individualist claims that all true theories of social science are *reducible*

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to theories of individual human action, plus boundary conditions specifying the conditions under which persons act" (1977, p. 353). He cites Hayek as holding this view. The problem with this statement is the potential ambiguity of the word *theory*. From the context, Nozick really means *hypotheses*. Nozick is not concerned with the problem of building a many-person model from the separate models of individuals (i.e., theories). Instead, he is concerned with the process of interpreting many-person interaction in terms of hypotheses about individuals. The point of methodological individualism in theory has nothing to do with whether actual interaction can be described solely in terms of individuals. Instead, as used by Mises, it deals with the question of whether a model of many-person interaction that is constructed by the economist, who is concerned only with human action, is logical. If it is logical, it must be reducible. If it is not reducible, it is not logical and must be jettisoned. Nozick apparently did not realize the limited scope of praxeology and economics, as Mises defined them.

11. Some of Caldwell's confusion might have been avoided by a more discriminating use of the word *theory*. The practice of equating theory with hypothesis has little to recommend it.

12. For a more complete discussion of Mises' distinction between theory and its application, see Mises' *Theory and History* (1969).

13. Mises did not regard the statistics as irrelevant in general. Indeed, to explain any particular historical event, one should use knowledge of all types, including knowedge of statistics. "In dealing with a historical problem the historian makes use of all the knowledge provided by logic, mathematics, the natural sciences, and especially by praxeology" (1966, p. 49).

14. It is not surprising that Caldwell himself did not recognize the difference in subject matter. *Human Action* is tough going for individuals who are trained in the positivism and falsificationism that has come to be the hallmark of reputable U.S. universities. It is significant to me, however, that the various "Austrians" listed as having commented in some way on Caldwell's paper apparently did not identify the difference in subject matter (1984, p. 377).

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Book Review Essays

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In Defense of Extreme Rationalism: Thoughts on Donald McCloskey's *The Rhetoric of Economics*

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The Relativism of Hermeneutics and Rhetoric and the Claims of Rationalism

For some time, the philosophy establishment has been under attack by the likes of Paul Feyerabend, Richard Rorty, Hans G. Gadamer, and Jacques Derrida. A movement of sorts that has already won over numerous members of the philosophy profession is steadily gaining ground, not only in such soft fields as literary criticism and sociology, but even in the hard natural sciences. With Donald McCloskey's *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985), this movement is ready to invade economics. Yet, it is not only the orthodox, neoclassical Chicago economist McCloskey who preaches the new dispensation; there is also G.L.S. Shackle, and at the fringes of the Austrian school of economics are Ludwig Lachmann and the George Mason University hermeneuticians who lend support to the new creed.

However, this creed is not entirely new. It is the ancient tune of skepticism and nihilism, of epistemological and ethical relativism that is sung here in ever-changing, modern voices. Richard Rorty, one of the outstanding champions of the creed, has presented it with admirable frankness in his *Philosophy and the Mirror of Nature.*[1] The opponent of the new old movement is *rationalism* and, in particular, *epistemology* as a product of rationalism. Rationalism, writes Rorty:

> is a desire for constraint—a desire to find "foundations" to which one might cling, frameworks beyond which one must not stray, objects which impose themselves, representations, which cannot be gainsaid. (p. 315)
>
> The dominating notion of epistemology is that to be rational, to be fully human, to do what we ought, we need to be able to find agreement with other

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Review of Donald McCloskey, *The Rhetoric of Economics* (Madison: University of Wisconsin Press, © 1985 by the Board of Regents of the University of Wisconsin System).

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> human beings. To construct an epistemology is to find the maximum amount of common ground with others. The assumption that an epistemology can be constructed is the assumption that such common ground exists. (p. 326)

However, Rorty claims that no such common ground exists: hence the false idol of rationalism must fall and a "relativist" position termed hermeneutics must be adopted.

> Hermeneutics sees the relations between various discourses as those of strands in a possible conversation, a conversation which presupposes no disciplinary matrix which unites the speakers, but where the hope of agreement is never lost so long as the conversation lasts. This hope is not a hope for the discovery of antecedently existing common ground, but *simply* hope for agreement, or, at least, exciting and fruitful disagreement. Epistemology sees the hope of agreement as a token of the existence of common ground which, perhaps unbeknown to the speakers, unites them in common rationality. For hermeneutics, to be rational is to be willing to refrain from epistemology—from thinking that there is a special set of terms in which all contributions to the conversation should be put—and to be willing to pick up the jargon of the interlocutor rather than translating it into one's own. For epistemology, to be rational is to find the proper set of terms into which all contributions should be translated if agreement is to become possible. For epistemology, conversation is implicit inquiry. For hermeneutics, inquiry is routine conversation. (p. 318)

What Rorty terms hermeneutics, McCloskey calls rhetoric. In *The Rhetoric of Economics*, he attempts to persuade us that in economics, just as in any other language game that we might play, rationalist and epistemological claims of providing a common ground that makes agreement-on-something-objectively-true possible are out of place. Economics, too, is merely rhetoric. It is another contribution to the conversation of mankind, another attempt to keep a routine going. It exists not for the sake of inquiring about what is true, but for its own sake; not in order to convince anyone of anything based on objective standards, but in the absence of any such standards, simply in order to be persuasive and persuade for persuasion's sake.

> Rhetoric is the art of speaking. More broadly it is the study of how people persuade. (p. 29)

> Rhetoric . . . is the box of tools for persuasion taken together, available to persuaders good and bad. (pp. 37–38)

> [Economics should learn its lesson from literary criticism.] "Literary criticism does not merely pass judgements of good or bad; in its more recent forms the question seems hardly to arise. Chiefly it is concerned with making readers see how poets and novelists accomplish their result. An economic criticism . . . is

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> not a way of passing judgement on economics. It is a way of showing how it accomplishes its result. It applies the devices of literary criticism to the literature of economics. (p. XIX)

> [The categories truth and falsehood play no role in this endeavor. Scholars] pursue other things, but things that have only an incidental relation with truth. They do so not because they are inferior to philosophers in moral fiber but because they are human. Truth-pursuing is a poor theory of human motivation and non-operational as a moral imperative. The human scientists pursue persuasiveness, prettiness, the resolution of puzzlement, the conquest of recalcitrant details, the feeling of a job well done, and the honor and income of office. . . . The very idea of Truth—with a capital T, something beyond what is merely persuasive to all concerned—is a fifth wheel. . . . If we decide that the quantity theory of money or the marginal productivity theory of distribution is persuasive, interesting, useful, reasonable, appealing, acceptable, we do not also need to know that it is True. . . . [There] are particular arguments, good or bad. After making them, there is no point in asking a last, summarizing question: "Well, is it True?" It's whatever it is—persuasive, interesting, useful, and so forth. . . . There is no reason to search for a general quality called Truth. (p. 46–47)

> [Economics in particular, and science in general are like the arts;[2] the law of demand is persuasive or unpersuasive in exactly the same way as a Keats poem;[3] and in just the same way as there exists no methodological formula for advancing artistic expression there exists none for advancing economics. Rhetoric] believes that science advances by healthy conversation, not adherence to a methodology. . . . Life is not so easy that an economist can be made better at what he does merely by reading a book. (p. 174)

Surely, after all this one has to catch one's breath. Yet has not rationalism refuted this doctrime time and again as self-contradictory and, if taken seriously, as fatally dangerous nonsense? Books such as McCloskey's may indeed not make life better or easier. But is this not only insofar as one ignores their advice; and would not life in fact be worse if one were actually to follow it?

Consider this: after reading Rorty and McCloskey, would it not seem appropriate to ask "What, then, about their own pronouncements?" If there is nothing like truth based on common, objective ground, then all of the preceding talk can surely not claim to say anything true. In fact, it would be self-defeating to do what they seem to be doing: denying that an objective case can be made for any statement, while at the same time claiming this to be the case for their own views. In so doing, one would falsify the content of one's own statement. One cannot argue that one cannot argue.[4] Thus, in order to understand Rorty and McCloskey correctly, one must first realize that they cannot truly be saying what they seem to be saying. Nor can I here say anything claiming to be objectively so and true. No, their talk as well as mine can merely be understood as contributions to their and my entertainment.

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But then, why should they or I listen and be entertained? After all, if there is no such thing as truth and, accordingly, no objective distinction between truth-claiming propositions and any others, then we are evidently faced with a situation of all-pervasive intellectual permissiveness.[5] With every statement just another contribution to the conversation of mankind, anything at all that is said is just as good a potential candidate for my entertainment as anything else. But why bother listening to such permissive, everything-goes talk? McCloskey might reply, "Because your talk or my talk is persuasive." But that will not change much, if anything at all. For according to his doctrine, the categories "persuasive" and "unpersuasive" are not simply other names for "true" and "false." The whole point would be lost if they were. No, he is saying that something is persuasive because it has in fact persuaded; because it has resulted in agreement. To go beyond this and ask, "Well, has one been persuaded of something correct?" would be an entirely inappropriate question. As a matter of fact, regarding any such question, he would have to point out that the very problem of determining whether or not a persuasion was based on correct talk would once more have to be decided on the actual persuasion of having been correctly persuaded; hence, that he is consistent in his rejection of the idea of objective truth; that the idea of breaking out of mere talk and of grounding talk in something that is not again simply talk is fallacious; and that truth then is itself no more than the subjective belief that what one believes is objectively true.[6] But if this is his position, then his talk, persuasive or unpersuasive as it may be, can indeed be no more than mere entertainment. Nor can this statement regarding what it means to talk claim to be objectively true; it, too, can only be meant to entertain.

Hence, it seems the first appropriate question regarding such books as McCloskey's would have to be "Are we being entertained?" Without a doubt, many a reader will reply that he is and McCloskey might then think that he has indeed achieved what he intended. But did he? Or was the readers' feeling of being well entertained only due to the fact that he misinterpreted what he read and understood it as something claiming to be true, which, in fact, it was never meant to be? And would not the reader, once he had realized this, have to change his opinion? For then McCloskey's talk clearly would not fall into any different category from that of a novelist or poet. But as compared with their prose, and in direct competition with any novel or poem written for our entertainment, I submit that McCloskey's book is merely boring and fails miserably in its objective.

Yet, can his book be even *bad* entertainment without still having to be committed indispensably to the notion of a common ground that serves as the basis of objective truth? Rationalism denies that it can. It claims that the notion of truth, of objective truth, of truth grounded in some reality outside that of language itself, is indispensable for talk of any sort, that language presupposes rationality, and hence that it is impossible to rid oneself of the notion of

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objective truth as long as one is capable of engaging in any language game whatsoever. For how else could we find out whether someone was in fact entertained by something, or that he was persuaded by it, that he understood or misunderstood what it was that had been said to entertain and persuade, and even further, whether there was something that meant anything at all and so could be understood, rather than merely being meaningless rustling in the wind? Clearly enough, we could not claim to know any of this unless we had a common language with commonly understood concepts such as "being persuaded" or "entertained" as well as any other term used in our talk. In fact, we could not meaningfully claim to deny all this without having to presuppose yet another set of commonly understood concepts. And just as clearly, this common ground that must be presupposed if we want to say anything meaningful at all is not simply one of free-floating sounds in harmony with each other in midair. Instead, it is the common ground of terms being used and applied cooperatively in the course of a practical affair, an interaction. And again, in making this claim, one could not possibly deny that this is so without presupposing that one in fact *could* cooperatively establish some common ground with respect to the practical application of some terms.

Language, then, is not some ethereal medium disconnected from reality, but is itself a form of action. It is an offshoot of practical cooperation and as such, via action, is inseparably connected with an objective world. Talk, whether fact or fiction, is inevitably a form of cooperation and thus presupposes a common ground of objectively defined and applied terms.[7] Not in the sense that one would always have to agree on the content of what was said or that one would even have to understand everything said. But rather, in the sense that as long as one claimed to express anything meaningful at all, one would have to assume the existence of *some* common standards, if only to be able to agree on whether or not and in what respect one was in fact in agreement with others, and whether or not and to what extent one in fact understood what had been said. And these common standards would have to be assumed to be objective in that they would involve the application of terms within reality. To say, then, that no common ground exists is contradictory. The very fact that this statement can claim to convey meaning implies that there is such common ground. It implies that terms can be objectively applied and grounded in a common reality of action as the practical presupposition of language.

Thus, if McCloskey were right and there were indeed no objective truth, he could not even claim to entertain anyone meaningfully with his book. His talk would be meaningless, indistinguishable from the rattling of his typewritter. He would advocate even greater intellectual permissiveness than first thought. Not only would he have to drop the distinction between truth-claiming propositions and propositions that merely claim to be entertaining, but his permissiveness would go so far as to disallow any distinction between meaningful talk and a meaningless assemblage of sounds. For one cannot even claim to

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entertain with talk that involves no truth-claim beyond that of being meaningful talk, without still having to know what objective truth is and be able to distinguish between truth-claiming propositions and those statements (for example, in fictional talk) that do not imply any such claim.

And there is more. For how can McCloskey or Rorty reconcile their view of science as mere talk with their own advocacy of a talk-ethic, an ethic described by McCloskey as follows:

> Don't lie [but how could we, if there were no such thing as objective truth? H.H.H.]; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence and conspiracy in aid of your ideas. (p. 24)

Why should we follow his advice of paying attention to talk and not resorting to violence, particularly in view of the fact that what is advocated here is talk of the sort where anything goes and where everything said is just as good a candidate for one's attention as anything else? It certainly is not evident that one should pay much attention to talk if that is what talk is all about! Moreover, it would be downright fatal to follow this ethic. For any viable human ethic must evidently allow people to do things other than talk, if only to have a single human survivor who could possibly have any ethical questions; McCloskey's talk-ethic, however, gives us precisely such deadly advice of never to *stop* talking or stop listening to others talk. In addition, McCloskey himself and his fellow hermeneuticians must admit that they can have no objective ground for proposing their ethic anyway. For if there are no objective standards of truth, then it must also be the case that one's ethical proposals cannot claim to be objectively justifiable either.[8] But what is wrong, then, with not being persuaded by all of this and, rather than listening further, hitting McCloskey on the head straightaway rather than waiting until he perishes from following his own prescription of endless talk? Clearly, if McCloskey were right, nothing could be said to be objectively wrong with this. (In fact, would one not have to conclude that McCloskey could not even say that anything objective had happened?) He might not regard my act of aggression as a contribution to the conversation of mankind (though we know by now that he could not even objectively claim to know this to be the case), but if the talk-ethic cannot itself be grounded in something objective outside of talk, then if I happened to be persuaded of an ethic of aggression instead, and I ended our conversation once and for all with a preemptive strike, McCloskey could not find anything objectively wrong with this either.

Thus, it is not only *intellectual* permissiveness that is preached by hermeneuticians and rhetoricians, it is total *practical* permissiveness as well—epistemological *and*, as the other side of the same coin, ethical relativism.[9] Yet such relativism is impossible to follow and thus wrong in the most objective

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sense of being literally incompatible with our nature as actors. Just as it is impossible to say and mean to say that there is no such thing as objective truth without in so doing actually presupposing objective criteria for the application of terms, so is it impossible to actually advocate ethical relativism. Because in order to advocate any ethical position whatsoever, one must be allowed to communicate rather than be coercively shut up and silenced, and thus, contrary to the relativist message itself, its messenger, in bringing it to us, must in fact presuppose the existence of objectively defined absolute rights. More specifically, he must presuppose those norms of action as valid whose observance makes talk as a special form of cooperation between physically separate talkers possible, while they must also allow everybody to do things other than engage in endless talk; and whose validity must then be regarded as objective and absolute in that no one could possibly ever be alive and talkingly challenge them.[10]

Hermeneutics versus Empiricism—Rationalism against Both
Round I

McCloskey's and Rorty's general thesis then, the very thesis that brought them their notoriety, is dead wrong. In fact, McCloskey and Rorty can only do and say what they do because *what* they say is false.

There is certainly much left to be said about rationalism, the age-old opponent of relativism. However, the perennial claims of rationalism remain unchallenged by this most modern, relativist attack: the claim that there exists a common ground on the basis of which objectively true propositions can be formulated; the claim that a rational ethic objectively founded in the nature of man as actors and talkers exists; and finally, the claim, only somewhat indirectly established in the previous argument and still to be substantiated, that one can know certain propositions to be objectively true a priori, (that is, independent of contingent experiences) as they can be derived deductively from basic, axiomatic propositions whose truth cannot be denied objectively without running into a practical contradiction, that is, without presupposing in the very act of denial what is supposedly denied (so that it would be literally impossible to undo the truth of these propositions).[11]

With this fundamental criticism out of the way, what about McCloskey's pronouncements, if for the sake of argument we are willing to ignore that he cannot really claim to say anything? It is not entirely surprising, as will be seen, that the general flaw of the book—its lack of argumentative rigor—also comes to bear here.

The very starting point of McCloskey's argument is marked by a misconception of the problem he faces. For in order to advance the thesis that economists

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should conceive of their jobs as keeping the conversation between economists going without ever claiming to say anything true (i.e., without ever supposing that anyone might ever have a decisive, conversation-stopping argument at his disposal), McCloskey would have to direct his argument against and refute the most extreme available opposition. He would have to choose as his target the claims of *rationalism* regarding the epistemological foundations and methodology of economics. And while only accounting for a small minority among today's theoreticians of economics, there surely exist some such dogmatic, doctrinaire, extremist, absolutist (or whatever other depreciating label one may choose) rationalists.[12] The foremost representatives of this persuasion are Ludwig von Mises and Murray N. Rothbard, who, within the general framework of a Kantian or, respectively, Aristotelian epistemology, conceive of economics as part of a pure theory of action and choice (praxeology).[13] Lionel Robbins advances only slightly less uncompromising views, in particular in the first edition of his *Nature and Significance of Economic Science.*[14] And from a very different position within the political-ideological spectrum are Martin Hollis and Edward J. Nell, who in their *Rational Economic Man* propound similar archrationalist claims regarding the logic of economics.[15] McCloskey would have to attack all of them, since they are the most radical conversation stoppers in that they all, despite some important differences, are completely uncompromising in insisting that economics not only can and *does* produce propositions that are objectively true and can be distinguished from propositions that are not, but, moreover, that some propositions of economics are grounded in incontestably true axioms or real (as contrasted with arbitrary, stipulative) definitions, and hence can be given an a priori justification.[16]

However, nowhere in his book does McCloskey attack these various representatives of an archrationalist methodology of economics, nor does he attack anyone else who falls into this camp. Nowhere in his book does he attack, much less refute, the very position that is the polar opposite of his. Robbins, Rothbard, Hollis, and Nell are never mentioned in McCloskey's text, nor do they appear in his bibliography. Nor does Mises' name appear in the bibliography, but it is mentioned twice in the text in support of some of McCloskey's own pronouncements (pp. 15, 65). Yet there is no reference to Mises' extremist, rationalist position. Austrian methodology is only cited in passing and described in a way that would strike anyone only faintly familiar with this intellectual tradition as no more than a naive misrepresentation: "Austrian methodology says: The history of all hitherto existing societies is the history of interactions among selfish individuals. Use statistics gingerly if at all, for they are transitory figments. Beware of remarks that do not accord with Austrian Methodological precepts" (p. 25).[17]

Rather than doing battle with his direct logical adversary, McCloskey chooses to establish his own relativist position through an attack on empiricism-positivism. But knocking down empiricism-positivism is no more than knocking

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down a straw man, in that from its downfall, absolutely nothing follows in support of McCloskey's own claims. In fact, all of the previously mentioned archrationalists have leveled much harsher criticism against empiricism-positivism and still apparently did not think that in so doing they would commit themselves to relativism. On the contrary, it is their view that any criticism of empiricism-positivism, if it is one that has any intellectual weight at all, would have to vindicate the very claims of rationalism. Thus, and this is the fundamental misconstruction of his entire argument, McCloskey, given his objective, simply fires at the wrong target and, worse, does not seem to notice.

However, as much as empiricism-positivism may deserve to be intellectually destroyed, McCloskey does not even succeed here. He begins with a description of empiricism-positivism or of economic modernism, as he terms the application of this philosophy to the field of economics, and lists its major precepts (pp. 7–8): prediction is what ultimately counts in science; there is no objective truth without observations; only quantifiable observations are objective data; introspection is subjective and worthless; science is positive and does not deal with normative questions; explaining something positively means bringing it under a general law; and a general law's validity is forever hypothetical, requiring permanent testing against objective observational data.

There is little to quarrel with regarding this characterization of modernism. Quite correctly, McCloskey also cites the most influential modern exponents of this creed: the Vienna Circle, analytical philosophy, and Popperianism in philosophy proper,[18] as well as such representative figures within the economics profession as T.W. Hutchison, Milton Friedman, and Mark Blaug.[19] And McCloskey is certainly correct, too, in identifying this modernist worldview as the current textbook orthodoxy. Nonetheless, from the outset, his understanding of empiricism-positivism is insufficient in that he fails to reconstruct the fundamental assumptions of modernism (i.e., those assumptions that underlie its various precepts). He neglects to assign them a specific place in a general, logically unified conceptual structure. He fails to clarify that the various specific modernist precepts flow essentially from the acceptance of one crucial assumption. The assumption, fundamental to modern empiricism, is that knowledge regarding reality, or empirical knowledge, must be verifiable or at least falsifiable by experience; that whatever is known by experience could have been otherwise, or, put differently, that nothing about reality could be known to be true a priori; that all a priori true statements are simply analytical statements that have no factual content, but are true by convention, representing merely tautological information about the use and the transformation rules of signs; that all cognitive meaningful statements are either empirical or analytical, but never both; and hence that normative statements, because they are neither empirical nor analytical, cannot legitimately contain any claim to truth, but must be regarded instead as mere expressions of emotions, saying in effect no more than "wow" or "grr."[20] And in failing to clarify this, McCloskey precipitates

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his subsequent failure to bring even empiricism-positivism, his chosen opponent, down. His attack is simply unsystematic, and it thereby necessarily misses its goal.

McCloskey's first criticism is well targeted. He shows that contrary to the claims of Popper and his school in particular, following the advice of the empiricist-falsificationist philosophy would ultimately lead one to skepticism. Whenever a hypothetical law is empirically tested and found to be lacking, within the very framework of an empiricist methodology it is always possible to immunize one's theory by denying the recalcitrant observations outright and declaring them illusory, by acknowledging them but ascribing their recalcitrance to measurement errors, or by postulating some unobserved, intervening variable, whose lack of control is to blame for the seemingly falsifying observations. Observes McCloskey:

> *Insulation from crucial test is the substance of most scientific disagreement.* Economists and other scientists will complain to their fellows, "Your experiment was not properly controlled"; "You have not solved the identification problem"; "You have used an equilibrium (competitive, single-equation) model when a disequilibrium (monopolistic, 500-equation) model is relevant." . . . There is no "falsification" going on. (p. 14)

And, he remarks further, have we not known since Thomas Kuhn's *Structure of Scientific Revolutions*[21] that the actual history of natural science does not seem to come anything close to the Popperian illusion of science as a rational enterprise steadily advancing through a never-ending process of successive falsification. "Falsification, near enough, has been falsified" (p. 15).

McCloskey also shows some understanding of the sociopsychology of modernist methodology: a philosophy such as empiricism, that starts with the assumption that nothing about reality can be known with certainty and hence everything is possible, and that has no place for anything such as objective a priori considerations; an epistemology, that is to say, that puts us under no constraints whatsoever when it comes to choosing our variables to be measured and determining the relation between such variables (except insofar as the chosen relation must fit the data), can be followed by almost everyone and almost everyone can justly feel that if this is what science is all about, he can be as good a scientist as anyone else. Anyone can measure whatever he feels like measuring, then with the help of a computer fit some curves or equations on his data material, and finally change or not change the curves or equations depending on new, incoming material and/or new hypotheses about measurement error or uncontrolled intervening variables. Empiricism is a methodology suited to the intellectually poor, hence its popularity.[22] Notes McCloskey:

> Graduate students in the social sciences view courses in econometrics, sociometrics, or psychometrics as courses in how to become applied economists,

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> sociologists, or psychologists. . . . The delusion is nourished by democracy, which partly explains its special prevalence in America. Everyone of normal intelligence can after such a course decipher the output of the Statistical Package for Social Sciences. No elite culture is necessary, no longer subordination to Doktor Herr Professor,[23] no knowledge accumulated through middle age. (p. 163)

Quite naturally, he sees all this as strong talk against modernist epistemology. And indeed, it might be enough to persuade someone to cease giving credence to modernism, and that would certainly be for the better. But even if true, does it constitute proof of a systematic flaw in the empiricist-positivist philosophy? And does it constitute proof in the hands of a hermeneutician?

As regards this latter question, it must be noted that for McCloskey himself to understand his statements about modernism as a *criticism* of this philosophy should strike one as simply odd. For in his discussion of empiricism-positivism, he clearly blames this philosophy for allowing scientists to engage in some all-too-pervasive intellectual permissiveness; for producing a science that advances nowhere but is a mere random walk of ideas through time to be understood only ex post by historical or sociological explanation; and thereby for opening the floodgates to the invasion of scholarship by intellectual barbarians. Yet McCloskey wants to replace this permissiveness with an even greater one. He wants us to engage in talk, endless and unconstrained by any intellectual discipline whatsoever. Thus, instead of criticizing empiricism-positivism, should he not embrace it enthusiastically for already coming so very close indeed to his own relativist ideals? If empiricism sounds ridiculous to McCloskey, his reason for this can only be that it is just not ridiculous enough, that empiricism is ridiculous because hermeneutics is even more so, and that pure nonsense must prevail over only partial nonsense.

Yet, apart from McCloskey's own position, his arguments directed against modernism cannot count as amounting to anything. "So what," the empiricist could reply. McCloskey has shown that following the modernist precepts leads to a peculiar form of relativism. Admittedly, some empiricists, most notably Popper and his school, have not and still do not recognize this.[24] McCloskey is right in pointing this out again. But then he must admit that this has also been realized by empiricists without causing them much intellectual pain. Was it not Feyerabend who first and most forcefully drove the relativist message home to Popperianism?[25] And was not he himself a leader of this very school who simply drew the ultimate logical conclusions of Popperianism?[26] Empiricism cannot explain the process of scientific development as a rational enterprise. True enough. But it cannot account for it because the process is not rational. And what is wrong with this? What is wrong with empiricism once it admits its own relativism?

McCloskey gives no answer to these questions. He does not advance any principled arguments that would prove empiricism to be a self-defeating

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position. Nor does he challenge empiricism on the much more obvious empirical front. It would seem to be evident that at least empiricism's claim of providing us with a correct epistemology of the natural sciences should, in view of the facts, be regarded as incorrect. For whatever the true state of affairs with respect to economics and the social sciences might be, with respect to the natural sciences it seems difficult to deny that hand in hand with their development went a steady, universally recognized process of technological advancement and improvement, and that this fact of technological progress can hardly be brought in line with the empiricist view of science as a relativistic, noncumulative enterprise. Empiricism then simply seems to have been *empirically* refuted as an appropriate methodology for the natural sciences.[27]

Yet such a refutation in no way supports McCloskey's own position. For the existence of technological progress would have to stand just as much in the way of hermeneutical relativism as in that of empiricism.[28] Only a rationalist methodology of the natural sciences could account for such progress. Only a methodology that begins with the recognition of the fact, as an undeniably true fact of our human nature as actors and talkers, that language in general and scientific theories in particular are ultimately grounded in a common, objective reality of action and cooperation can explain why such progress is possible without thereby having to deny some partial correctness of Kuhn and Feyerabend's relativistic portrayals of the history of the natural sciences.

The relativistic impression is due to the fact that Kuhn and Feyerabend, typical of empiricists since Locke and Hume, ultimately misconceive of scientific theories as mere systems of verbal propositions and systematically ignore the foundation of these, or of any, propositions in a reality of action and interaction.[29] Only if one regards observations and theories as being completely detached from action and cooperation, not only does any single theory become immunizable, but any two rival theories whose respective terms cannot be reduced to and defined in terms of each other must then appear completely incommensurable and no rational choice is possible. If statements are merely and exclusively verbal expressions hanging in midair, what reason could there be for any one statement to ever give way to another? Any one statement can perfectly well stand alongside any other one without ever being challenged—unless we simply decide otherwise for whatever arbitrary reason. It is this that Kuhn and Feyerabend demonstrate. But this does not affect the refutability of any one theory and the commensurability of rival theories on the entirely different level of applying these theories in the reality of action, of using them as instruments of action. On the level of mere words, theories may be irrefutable and incommensurable, but practically they can never be. In fact, one could not even state that any single theory was irrefutable or any two theories were incommensurable and in what respect, unless one were to presuppose a common categorical framework that could serve as a basis for such an assessment

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or comparison. And it is this *practical* refutability and commensurability of theories of natural science that explains the possibility of technological progress—even though it accounted for technological progress in quite a different manner than Popper's failed attempt.[30]

Popper would have us throw out any theory that is contradicted by any fact, which, if at all possible, would leave us virtually empty-handed, going nowhere. In recognizing the insoluble connection between theoretical knowledge (language) and actions, rationalism would instead deem such falsificationism, even if possible, as completely irrational. There is no situation conceivable in which it would be reasonable to throw away any theory—conceived of as a cognitive instrument of action—that had been successfully applied in a past situation but proves unsuccessful in a new application—unless one already had a more successful theory at hand. And to thus immunize a theory from experience is perfectly rational from the point of view of an actor. And it is just as rational for an actor to regard any two rivals, in their range of application overlapping theories t_1 and t_2 as incommensurable as long as there exists a single application in which t_1 is more successful than t_2 or vice versa. Only if t_1 can be as successfully applied as t_2 to every single instance to which t_2 is applicable but still has more and different applications than t_2 can it ever be rational to discard t_2. To discard it any earlier, because of unsuccessful applications or because t_1 could in some or even in most situations have been applied more successfully, would from the point of view of a knowing actor not be progress but retrogression. And even if t_2 is rationally discarded, progress is not achieved by falsifying it, as t_2 would actually have had some successful applications that could never possibly be nullified by anything (in the future). Instead, t_1 would outcompete t_2 in such a way that any further clinging to t_2, though of course possible, would be possible only at the price of not being able to successfully do everything that an adherent of t_1 could do who could successfully do as much and more than any proponent of t_2.

Trivial as such an account of the possibility of progress (as well as retrogression) in the natural sciences may seem, it is incompatible with empiricism. In systematically ignoring the fact that observations and theories are those of an actor, made and built in order to act successfully, empiricism has naturally deprived itself of the very criterion against which knowledge is continually tested and commensurated: the criterion of successfully or unsuccessfully reaching a set goal in applying knowledge in a given situation.[31] Without the explicit recognition of the universal operativeness of the criterion of instrumental success, relativism was inescapable. However, such relativism would once more literally be impossible to adopt, because it is incompatible with our nature as acting talkers and knowers. Relativism could not even meaningfully claim to deny the operativeness of this criterion, as this very denial would itself have to be an action that presupposed some objective standard of success. Rather, in each of our actions, we confirm rationalism's claim (as regards the natural

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sciences) that one can objectively identify a range of applications for some knowledge and then test it for its success within this range, and, hence, that competing theories must be considered commensurable as regards such ranges of applications and success.

Hermeneutics versus Empiricism—Rationalism against Both Round II

McCloskey's first round against empiricism then is a complete failure. Nor is his second round of criticism any more successful. There, McCloskey takes issue with the modernists' emphasis on prediction as the cornerstone of science. Though he does not deny the possibility of prediction in the natural sciences, he doubts its overwhelming importance. However, prediction in economics, he claims, is impossible. "Predicting the economic future is, as Ludwig von Mises put it, 'beyond the power of any mortal man' " (p. 15).

In order to defend this thesis, we would expect him to establish two separate but related claims. First would be the claim that something is wrong with methodological monism—the program of an *Einheitswissenschaft*—and methodological dualism should be adopted. Otherwise it makes no sense to say that predictions are possible in one field of inquiry but impossible in another. The second claim would be that on the basis of such a dualist position, it can be demonstrated why predictions are possible in one field but not in another. McCloskey does nothing of this sort. It entirely escapes his notice that his position vis-à-vis modernism requires him to attack empiricism on account of its monism; that its monist stand makes it actually impossible for empiricism to explain how predictions, which allegedly constitute the very heart of the empiricist program, can conceivably be possible—and impossible for precisely the same reason that empiricism could not account for the possibility of progress in natural science; and that a dualist position (which McCloskey would be required to take if he wanted to systematically challenge modernism) would be incompatible with hermeneutics—itself being a monist position, though a different sort than empiricism's—and can again only be reconciled with a rationalist methodology, which alone can account for the possibility of the empiricist dream of predictions.

Empiricism is observational monism, stating that all our empirical knowledge is derived from observations and consists in interrelating these observations; and, further, that observations as well as relations have the permanent status of only being true hypothetically. This is the case in economics as well as in any other field concerned with empirical knowledge, and hence the problem of prediction must be the same everywhere. McCloskey does not answer this systematic challenge. He does not present the conclusive refutation

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of such monism by pointing out that in claiming what empiricism claims, one in fact falsifies the content of one's statement. For to claim what it claims, empiricism must actually presuppose that in addition to observations, meaningful objects exist—words tied to reality via cooperation—that, along with the relations among them, must be understood rather than observed. Hence the need for methodological dualism.[32]

Nor does McCloskey notice the incompatibility of observational monism with the notion of prediction. The idea of prediction and causality (i.e., that there are constant, time-invariantly operating causes that allow one to project past observations regarding the relationship between variables into the future) is something (as empiricism since Hume has noticed) that has no observational basis and hence cannot be said to be justified (within the empiricist framework). One cannot observe the connecting link between observations, except that they are somehow contingently related in time. And even if one could observe it, this observation would still not prove that such an observed connection was time-invariant. Strictly speaking, within the framework of observational monism, it does not even make sense to place observations in objective time.[33] Rather, the observed relationships are those between data in the temporal order in which an observer happens to observe them (clearly something very different from our notion of being able to distinguish between a real, causally effective order and sequence of observations and the mere temporal order in which observations are made). Hence, strictly speaking, according to empiricism, predictions are epistemologically impossible. It is irrational to want to predict, because the very possibility of prediction cannot be rationally established. And this, then, is also the ultimate reason for empiricism's skeptical stand regarding the possibility of scientific progress. For if one cannot rationally defend the very idea of causality, how can one expect anything from science but an array of incommensurable observational statements? Progress, as it is commonly understood, is the advancement of predictive knowledge. But surely no such thing can be possible if prediction itself cannot be established as possible.[34]

McCloskey also does not confront the challenge of explaining how hermeneutics accounts for a dualism and the very possibility of prediction (if only in the natural sciences). Nor could he have succeeded in this. For an argument such as dualism would establish that certain propositions can be said to be objectively true—in fact to be a priori true—and this would contradict the relativist message of hermeneutics. Yet as a monist position, hermeneutics cannot account for causality any more than empiricism can. As an observational monism, empiricism would like to reduce all our empirical knowledge to observations and observations of contingent relations between observations, and it is therefore ultimately forced to abandon the idea of time-invariantly operating causes. Hermeneutics would like to reduce it to a talk-monism; to talk disconnected to anything real outside of talk itself; to sequences of talk hanging in midair with no objective, talk-constraining grounding whatsoever. For this

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reason, hermeneutics cannot account for causality. For in the absence of any common, objective standard, all talk is simply incommensurable, and no objective connection whatsoever can exist between any talk apart from the mere temporal order of talking.

Both dualism and causality can only be explained by rationalism. Rationalism begins with the insight that empiricism is self-refuting, since it cannot actually state its own position without implicitly admitting that in addition to observations and contingent relations of observations, other meaningful things and relations (i.e., words sustained through action and acquiring meaning in the course of such action) must also exist. Similarly, rationalism rejects hermeneutics as self-refuting, because a talk-monism, too, could not be stated without implicitly admitting it as false in that it would have to presuppose the very existence of actions guided by observations, if only in order to sustain talk—thus falsifying the claim of talk ever being unconstrained by anything objective. And rationalism then concludes that the key to the problem of causality must lie in the recognition of the fact (ignored by both empiricism and hermeneutics) that observations as well as words are constrained by action, and that this can be established neither by observation nor by mere talk, but rather must be understood on account of our knowledge of action as the practical presupposition of any observation or talk as an a priori true fact of human nature.

It is from such a priori understanding of action that the idea of causality can indeed be derived.[35] Causality is not a category of observations. It is a category of action whose knowledge as an a priori feature of reality is rooted in our very understanding of our nature as actors. Only because we are actors and our experiences are those of acting individuals can observations be conceived of as occurring objectively earlier or later and as being related to each other through time-invariantly operating causes.[36] No one who did not know what it meant to act could ever experience events occurring in real time and in invariant causal sequences. And no one's knowledge of the meaning of action and causality could ever be said to be derived from contingent observational evidence, as the very fact of experiencing already presupposes action and causally interpreted observations. Every action is and must be understood as an interference with the observational world, made with the intent of diverting the "natural" course of events in order to produce (i.e., to *cause* to come into being) a different, preferred state of affairs—of making things happen that otherwise would not happen—and thus presupposes the notions of events placed in objective time and of time-invariantly operating causes. An actor can err with respect to his particular assumptions about which earlier interference produced which later result, and thus his interference might not actually turn out to be successful. But successful or not, any action, changed or unchanged in light of its success or failure, presupposes that there are constantly connected events in time, even if no particular cause for any particular event can ever be

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preknown to any actor at any time. In fact, attempting to disprove that observational events are governed by time-invariantly operating causes would require one to show that some given event cannot be observed or produced based on some earlier interference. Yet trying to disprove this would again necessarily presuppose that the occurrence or nonoccurrence of the phenomenon under scrutiny could, in fact, be effected by taking appropriate action, and that the phenomenon must thus presumably be embedded in a network of constantly operating causes. Hence, rationalism concludes that the validity of the principle of causality cannot be falsified by taking any action, since any action would have to presuppose it.[37]

McCloskey notices none of this. And so it is no surprise that the arguments raised in support of his claim regarding the impossibility of prediction in economics are off the mark, too. Though in themselves correct arguments, they simply do not constitute the impossibility theorem that is needed.

What McCloskey offers as proof, which he incidentally claims to be "more precise" than some earlier, related Austrian thoughts (p. 90), is the following insight: "If economists could do [predict] better than business people, the economists would be rich. They are not" (p. 93). Hence, we should not trust people who claim to have information about future economic events. For if they really did have such knowledge, why would *they* not strike it rich, instead of telling *us* how to do it (p. 16)? Realistically, we should regard economic forecasters as providing information that, generally speaking, is economically worthless in that it tells us no more about future economic events than what concerned people on the average believe and expect anyway and have already accounted for in their present actions (p. 93f.).

Good enough. However, a much more succinct presentation than this can already be found in Mises.

> There are no rules according to which the duration of the boom or of the following depression can be computed. And even if such rules were available they would be of no use to businessmen. What the individual businessman needs in order to avoid losses is knowledge about the date of the turning point at a time when other businessmen still believe that the crash is farther away than is really the case. . . . Entrepreneurial judgement cannot be bought on the market. The entrepreneurial idea that carries on and brings profit is precisely that idea which did not occur to the majority. It is not correct foresight as such that yields profits, but foresight better than that of the rest.[38]

Yet this, as Mises but not McCloskey knows, does not prove the impossibility of causal predictions in economics.[39] All it proves is that differential profits can only emerge from *differences* in knowledge. The question is, however, if such knowledge—regardless of whether it is unequally distributed and thus allows for the possibility of differential profits and losses, or equally distributed and thus tends to only account for a uniform rate of return for the forecasters—is

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such that it could be expressed in a prediction formula that could legitimately make use of the assumption of time-invariant causes and hence could be conceived of as a systematically testable and improvable formula.

Surely McCloskey does not want to deny the possibility of prediction as such in economics. We constantly make such predictions. Moreover, while economic forecasters may not generally be rich and thus evidently may not know more than the rest of us, some of them are, and certainly there are some businessmen who are rich. Evidently, people not only can forecast, but can forecast correctly and successfully. The impossibility theorem cannot be meant to prove that no (successful) prediction whatsoever can be made in the field of economics, but rather only that a certain type of prediction is impossible here that is possible elsewhere. Yet the argument does not prove this. For we have no difficulties applying the idea of differential predictive knowledge and differential returns from forecasting to the field of the natural sciences, and still conceiving of them as gradually progressing and producing ever-improved prediction formulae. One natural-science forecaster may know more than another and even stay ahead of the competition permanently, but this does not imply that his relative advantage is not one that could not possibly be expressed, at all times, in terms of a formula that uses predictive constants and is capable of systematic improvement by means of successive testings. Why, then, should this be any different in the realm of economic forecasting? Why can the rich businessman not have gained his position in the same way as the relatively more successful natural-science forecaster?

This is what must be answered by the impossibility theorem. On this, however, McCloskey is silent. Nor can an answer be formulated by a hermeneutician. For an impossibility theorem would be precisely the kind of conversation-stopping argument that McCloskey claims to be nonexistent. To prove that economic forecasting is categorically different from natural-science forecasting would only mean confirming the claims of rationalism. Such proof would not have relativistic consequences regarding economic predictions as it may at first seem—such as to say that no systematic mistake whatsoever could be made by an economic forecaster and that any economic forecast's failure or success would thus be due entirely to bad or good luck. Instead, even if it were to show that there were indeed some ineradicable element of luck in economic forecasting, making progress as it exists in technological forecasting impossible in the field of economics, at the same time such proof would establish the existence of a priori true propositions on the subject matter of economics, which would then systematically constrain the range of possible predictions about future economic events and open up the possibility of predictions that were systematically flawed in that they would be at variance with such fundamental, a priori valid knowledge.

And indeed, argues rationalism, economic predictions that would make use of the assumption of time-invariantly operating causes must thus be considered

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systematically flawed.[40] While every action presupposes causality, no actor can conceive of his actions as ever being predictable on the basis of constantly operating causes. Causality can only be assumed to exist outside of the field of human action, and economic predictions as predictions concerning future actions are impossible. This follows from the very modernism that McCloskey criticizes, incidentally proving this position a self-refuting one once again. Empiricism claims that actions, just as any other phenomenon, can and must be explained by means of causal hypotheses that can be confirmed or falsified by experience. Now, if this were the case, empiricism would be forced to assume—contrary to its own doctrine that there is no a priori knowledge about reality—that time-invariantly operating causes with respect to actions exist. One would not know a priori which particular event might be the cause of a particular action. Experience would have to reveal this. But in order to proceed in the way that empiricism wants us to proceed (i.e., to relate different experiences regarding sequences of events as either confirming or falsifying each other and, if falsifying, then responding with a reformulation of the causal hypothesis), a constancy over time in the operation of causes *as such* must be presupposed. (Without such an assumption, the different experiences would simply be unrelated, incommensurable observations.[41]) However, if this were true, and actions could indeed be conceived of as governed by time-invariantly operating causes, what about explaining the explainers (i.e. the persons who carry on the very process of creating hypotheses), of verification and falsification? Evidently, in order to assimilate confirming or falsifying experiences—to replace old hypotheses with new ones—one must presumably be able to learn. However, if one is able to learn from experience, then one cannot know at any given time what one will know at a later time and how one will act based on this later knowledge. Rather, one can only reconstruct the causes of one's actions after the event, as one can only explain one's knowledge after one already possesses it. Thus, the empiricist methodology applied to the field of knowledge and action, which contains knowledge as its necessary ingredient, is simply contradictory—a logical absurdity.[42]

Moreover, it is plainly contradictory to argue that one could ever predict one's knowledge and actions based on antecedent, constantly operating causes. For to argue so is not only absurd because it implies that one can know now what one will know in the future; it is also self-defeating, because to do so would actually be saying that there was something that was not yet understood, but rather had to be learned about and examined as regards the acceptability of its validity claims, with as yet unknown results with respect to the outcome of this (either for our future kowledge, or for our and others' knowledge about the knowledge of others).

Thus, as McCloskey states yet does not prove, causal empirical explanations regarding knowledge and actions are indeed impossible. Whoever pretends, as empiricist economists invariably do, to be able to predict future knowledge

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and actions based on constantly operating antecedent variables is simply speaking nonsense. There are no such constants in the field of human action, as Mises insisted over and over again. Economic forecasting is not and never can be a science, but will always be a systematically unteachable art. Yet, and I shall return to this shortly, this does not mean that such forecasts would not be constrained by anything. While no particular action can ever be predicted scientifically, each and every prediction of future actions and the consequences of actions is constrained by our a priori knowledge of actions as such.

Rationalism and the Foundations of Economics

In the second round of its criticism of empiricism-positivism, hermeneutics fails just as it failed in the first. And again it is philosophical rationalism—equally critical of hermeneutics and empiricism—that is vindicated. Yet McCloskey makes one more point worth mentioning, as he reminds us that modern hermeneutics is an outgrowth of the discipline of interpreting the Bible.[43] In line with this traditionalist orientation, the case for hermeneutics ultimately boils down to an uncritical appeal to and acceptance of authority. We are asked by McCloskey to embrace the new old creed because some authorities tell us to do so. In his view, empiricism is not wrong as such—as a matter of fact, there was a time when it was quite all right to follow empiricist advice. But that was when philosophical authorities were all sold on empiricism. In the meantime, empiricism is out of favor with the philosopher kings and only the practitioners of science still cling to it—not realizing that fashion has changed. It is high time, then, that we shift and follow the new trend setters. Writes McCloskey: "The argument that Hutchison, Samuelson, Friedman, Machlup, and their followers gave for adopting their metaphysics was an argument from authority, at the time correct, namely that this was what philosophers were saying. The trust in philosophy was a tactical error, for philosophy itself was changing as they spoke" (p. 12). And the same goes for the mathematization of economics. Once it was good; now it is becoming bad. The winds of fashion change and we had better pay attention to this. "Economists before the reception of mathematics fell headlong . . . into confusions that a little mathematics would have cleared up." Imagine, they

> could not keep clear, for instance, the difference between a movement of an entire curve and a movement along a curve. . . . But now, so long after the victory, one might ask whether the faith which supported it still serves a social function. One might ask whether the strident talk of Science in economics, which served well in bringing clarity and rigor to the field, has outlived its usefulness." (pp. 3–5)

Surely, this lives up again to truly relativistic form. Yet as we have seen, there is no reason in the world to accept such relativism. Relativism is a self-

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contradictory position. And just as it is impossible to defend the hermeneutical relativism as the methodology of today, so is it impossible to defend the empiricism-positivism of yesterday. Empiricism-positivism, too, is a self-defeating doctrine, and not only because of its observational monism, which cannot be stated without implicitly admitting its falsehood and accepting a dualism of observable and meaningful phenomena to be understood on account of our knowledge of action and cooperation. Empiricism's fundamental distinction between analytical, empirical, and normative propositions is equally indefensible. What then is the status of the very proposition introducing this distinction? Assuming that empiricist reasoning is correct, it must be either an analytical or an empirical proposition, or it must be an expression of emotions. If it is understood as analytical, then according to its own doctrine it is merely verbal quibble, saying nothing about anything real but rather only defining one sound or sign by another, and hence one would simply have to reply "so what?" The same response would be appropriate, if, instead, the basic empiricist proposition were taken to be an empirical one. For if this were so, it would not only have to be admitted that the propositions might well be wrong. More decisively, as an empirical proposition, the most it could state would be a historical fact and it would thus be entirely irrelevant in determining whether or not it would be *impossible* to *ever* produce either a priori true propositions that were not analytical or normative propositions that were not emotive. And finally, if the empiricist line of reasoning were assumed to be an emotive argument, then according to its own pronouncements, it is cognitively meaningless and one would not have to pay any more attention to it than to a dog's bark. Thus, one must again conclude that empiricism-positivism is an utter failure. If it were correct, its basic premise could not even be stated as a cognitively meaningful proposition; and if it could be so stated and empiricism were indeed making the proposition that we all along thought it did, then the analytical-empirical-normative distinction would be proven false by the very proposition introducing it.[44]

How then, could it ever have been right to follow a false doctrine? To conceive of economics, or more precisely of actions, as empiricism does, and accordingly to treat economic phenomena as observable variables, measurable and tractable by mathematical reasoning, must have always been wrong. And the surge of positivism in economics could never have added to clarity, but from the very beginning must have helped instead to introduce ever more falsehoods into the field.

There *is* empirical knowledge that is valid a priori. And such knowledge informs us that it has never been correct to represent relationships between economic phenomena in terms of equations containing the assumption of empirical causal constants, because to conceive of actions as being caused by and predictable on the basis of antecedent variables is contradictory. Moreover, the very same a priori knowledge reveals that it is at all times incorrect to conceive

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of economic variables as observable magnitudes. Rather, all categories of action must be understood as existing only as subjective interpretations of observable events. The fact that knowledge and talk are those of an actor and constrained by our nature as actors cannot be observed, but rather must be understood. Nor can causality or objective time ever be simply observed, but our knowledge of it is based on our prior understanding of what it is to act. And so it is regarding the rest of the economic categories, as Mises above all has shown. There are no values to be observed, but things can be understood as valued only because of our prior knowledge of action. As a matter of fact, that there is such a thing as actions also cannot be observed, but must be understood. It cannot be observed that with every action, an actor pursues a goal and that whatever his goal, the fact that it is pursued by an actor reveals that he places a relatively higher value on it than on any other goal of action that he at the very start of his action could think of. Further, it can neither be observed that in order to achieve his most highly valued goal an actor must interfere (or decide not to interfere) at an earlier point in time to produce some later result, nor that such interferences invariably imply the employment of some scarce means (at least those of the actors' body, its standing room, and the time absorbed by the interference). It is unobservable (1) that these means must also have value for an actor—a value derived from that of the goal—because the actor must think their employment necessary in order to effectively achieve the goal and (2) that actions can only be performed sequentially, always involving the making of a choice (i.e., taking up that course of action that at some given point in time promises the most highly valued result to the actor and excluding at the same time the pursual of other, less highly valued goals). It cannot be observed that as a consequence of having to choose and give preference to one goal over another—of not being able to realize all goals simultaneously—each and every action implies the incurrence of costs (i.e., forsaking the value attached to the most highly valued alternative goal that cannot be realized or whose realization must be deferred because the means necessary to effect it are bound up in the production of another even more highly valued end). And lastly, it is unobservable that at its starting point, every goal of action must be considered (1) worth more to the actor than its costs and (2) capable of yielding a profit (i.e., a result whose value is ranked higher than that of the forgone opportunities, and yet that every action is also invariably open to the possibility of a loss if an actor finds, in retrospect, that the actually achieved result—contrary to previous expectations—in fact has a lower value than the relinquished alternative would have had.

All of these categories (values, ends, means, choice, preference, cost, profit and loss, time, and causality) are implied in the concept of action. That one is able to interpret experiences in such categories requires that one already knows what it means to act. No one who is not an actor could ever understand them, as they are not "given," ready to be experienced, but experience is cast in these

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terms as it is constructed by an actor. And then to treat such concepts, as empiricism-positivism would, as things extending in space and allowing quantifiable measurements is missing the goal entirely. Whatever one might explain in following empiricist advice, it has nothing whatsoever to do with explaining actions and experiences cast in the categories of action. These categories are ineradicably subjective ones. And yet they represent empirical knowledge in that they are conceptual organizations of real events and occurrences. They are not merely verbal definitions; they are real definitions of real things and real observations.[45] Furthermore, they are not only empirical knowledge; contrary to all relativistic aspirations, they incorporate a priori valid empirical knowledge. For it would clearly be impossible to disprove their empirical validity, as the attempt to do so would itself be an action aimed at a goal, requiring means, excluding other courses of action, incurring costs, and subjecting the actor to the possibility of achieving or not achieving the desired goal and so making a profit or suffering a loss. The very possession of such knowledge can never be disputed, and the validity of these concepts can never be falsified by any contingent experience, since disputing or falsifying anything already presupposes its very existence. As a matter of fact, a situation in which these categories of action would cease to have a real existence could itself never be observed, as making an observation is in itself an action.

Economic reasoning has its foundations in this a priori knowledge of the meaning of action.[46] It concerns phenomena that, though existing objectively, cannot be subjected to physical measurements, but must be *understood* as conceptually distinct events. And it concerns phenomena that cannot be predicted based on constantly operating causes; and our predictive knowledge about such phenomena, accordingly, cannot be said to be constrained by contingent empirical laws (i.e., laws that one would have to discover through a posteriori experiences). Instead, it concerns objects and events that are constrained by the existence of a priori valid, logical, or praxeological laws and constraints (i.e., laws whose validity is completely independent of any kind of a posteriori experience). Economic reasoning consists of (1) an understanding of the categories of action and the meaning of a change in values, preferences, knowledge, means, cost, profit, or loss, and so on, (2) a description of a situation in which these categories assume specific meaning and definite individuals are described as actors, with definite things specified as their goals, means, profits, and costs, and (3) a logical deduction of the consequences which result from the introduction of some specified action in this situation, or of the consequences which result for an actor if this situation is changed in a specified way. Provided there is no flaw in the process of deduction, the conclusions that such reasoning yields are valid a priori because their validity would ultimately go back to the indisputable axiom of action. If the situation and the changes introduced into it are fiction or assumptions, then the conclusions are true a priori only of a possible world. If, on the other and, the situation and situational

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changes can be identified as real, perceived, and conceptualized as such by real actors, then the conclusions are a priori true propositions about the world as it really is. And such realistic conclusions, which are the economists's main concern, act as logical constraints on our actual predictions of future economic events. They do not guarantee correct predictions—even if the empirical assumptions are indeed correct and the deductions are flawless—because in reality, there can be all sorts of situational changes happening concurrently or following the explicitly introduced change in the action-world data. And though they also affect the shape of things to come (and cancel, increase, decrease, accelerate, or decelerate effects stemming from other sources), such concurrent changes can in principle never be predicted or experimentally held constant, because to conceive of subjective knowledge (whose every change has an impact on action) as predictable on the basis of antecedent variables and as capable of being held constant is an outright absurdity. The experimenter who so wanted to hold it constant would in fact have to presuppose that his knowledge, specifically his knowledge regarding the experiment's outcome, could not be assumed to be constant over time. However, while they cannot render any specific future economic event certain or even predictable on the basis of a formula, such a priori conclusions nonetheless systematically restrict the range of possibly correct predictions. Predictions that are not in line with such knowledge would be systematically flawed and would lead to a systematically increased number of forecasting errors—not in the sense that anyone who based his predictions on correct praxeological reasoning would necessarily have to be a better predictor of future economic events than someone who arrived at his predictions through logically flawed deliberations and chains of reasoning, but in the sense that in the long run, certeris paribus, the first group of forecasters would average a better record than the second.

Regarding any specific forecast, it is very possible to falter despite one's correct identification of a situational change as described in terms of the a priori categories of action and one's correct analysis of the praxeological consequences of such change, because one might err regarding one's identification of other, accompanying changes. It is equally possible to arrive at a correct forecast in spite of the fact that one's inferences drawn from one's correct description of a situational change were praxeologically wrong, because other concurrent events might be of such a kind as to counteract such a wrong assessment of consequences. However, if it is assumed that, on the average, forecasters with or without a solid grasp of praxeological laws and constants are both equally well equipped to anticipate such other concurrent changes in the action-world and to account for them in their predictions, then the group of forecasters that makes its predictions in recognition of and accordance with such laws will be more successful than that which does not.

As are all economic theorems, the law of demand (which causes empiricists as well as hermeneuticians considerable uneasiness because of its apodictically

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assumed central position in economics) is an a priori true constraint on actual predictions regarding the consequences of certain actions. Empiricism tells us to conceive of it as an in-principle falsifiable hypothesis about the consequences of price changes. Yet, if we accept this and empirically test the law, we frequently find that a price increase, for instance, goes hand in hand with an increase in the quantity demanded, or that a price decrease is accompanied by a reduced demand. The law holds sometimes and for some goods, but at other times, for the same or other goods, it does not. How then, concludes empiricism, can economists assign to this law the axiomatic position that it occupies in economic theory and build a complex network of thought based on it? To do so must seem to an empiricist to be nothing but bad metaphysics that needs to be expelled from the discipline as soon as possible in order to bring economics back onto the right track.[47]

Hermeneutics is no more successful in justifying the law of demand. McCloskey realizes that the empiricist case for the law is weak at best. Yet he believes it acceptable to stick with it—as, despite their professed empiricism, most economists indeed do—because the law of demand is allegedly persuasive in light of other hermeneutical evidence (pp. 58–60). Such supportive evidence supposedly comes from "introspection," from "thought experiments," and from illustrative case stories; there is the persuasive fact that "business people" believe in the law, and "many wise economists"; the "symmetry of the law" makes it esthetically appealing; "mere definition" adds power; and "above all, there is analogy. That the Law of Demand is true for ice cream and movies, as no one would want to deny, makes it more persuasive also for gasoline" (p. 60). None of this, however, could make the law of demand any better founded and give it the authority it indeed seems to command. To be sure, introspection is the source of our knowledge of the law of demand. This particular law is no more founded in observations than are the laws of logic and mathematics. Yet introspections as such, or thought experiments, can no more establish the law of demand than can observational evidence. Introspective evidence, too, is nothing other than contingent experience. Here and now somebody arrives at this thought, and there and then someone else reaches the same or a different one. As McCloskey himself states, "if properly socialized in economics," introspection and thought experimentation make the law highly persuasive (p. 59). But, mutatis mutandis, then, if one is not so socialized, introspection might render the law far less appealing. Then, however, introspection as such can hardly be said to lend any systematic support to it. In fact, to appeal to the economists' introspective evidence would amount to a begging of the question, as it would have to be explained why one should accept this economic socialization or brainwashing in the first place. In the same way, case stories or convictions of certain businessmen or wise economists are not proof of anything. Aesthetic criteria and mere definitions, too, have no epistemological value. And conclusions per analogiam are only conclusive if the analogy itself can be said

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to be correct—besides the fact that it would certainly not be impossible for someone to say that the law of demand sounds unpersuasive even for ice cream and movies.[48] Hence, hermeneutics offers nothing substantive to vindicate our belief in the law of demand.

And yet the law of demand is objectively true despite the fact that it is not based on contingent external or internal experiences. Rather, its foundation lies in our introspective understanding of action as the practical presupposition of our external as well as our internal experiences and in the recognition of the fact that this understanding must be considered epistemologically prior to any contingent act of understanding in that it could not possibly be falsified by it. The fact that in order to exchange successive units of a good A for successive units of a good B, the exchange ratio of A to B must fall follows from the law of marginal utility: as the supply of A decreases and the marginal utility of a unit of A increases, the supply of B increases and B's marginal utility decreases, and hence successive units of A will become exchangeable for successive units of B only if counteracting these divergent changes in the valuation of As and Bs that follow each exchange, B becomes successively cheaper in terms of A. And as the foundation of the law of demand, this law of marginal utility then follows directly from the undeniably true proposition that every actor always prefers what satisfies him more over what satisfies him less.[49] For then any increase in the supply of a homogeneous good (i.e., a good whose units are considered to be interchangeable and of equal serviceability) by one additional unit can only be employed as a means for the attainment of a goal that is considered less valuable (or the removal of an uneasiness that is deemed less urgent) than the least valuable goal satisfied by means of a unit of such a good if the supply were one unit less.[50] And, as required of any a priori law and again independent of any contingent experiences, this law also precisely delineates its range of application and explains what possible occurrences cannot be considered exceptions or falsifying events. For one thing, the validity of the law of diminishing marginal utility is not at all affected by the fact that the utility of the marginal unit of some good can increase as well as decrease over time. If, for instance, a hitherto unknown use for a unit of some good is found that is considered more valuable than the least urgent present use of a unit of this good, the utility derived from the marginal employment would be higher now than previously. Yet despite such an increase in marginal utility, there is no question of such a thing as a law of increasing marginal utility. For not only would the actor whose supply of the good in question was unchanged and who realized such new employment have to give up some previously satisfied desire in order to satisfy another one; he would give up the less urgent one. Moreover, if with this new state of affairs regarding an actor's knowledge about possible employments for units of some given good, its supply increases by an additional unit, its marginal utility would decrease as it would be employed to satisfy precisely that desire that previously had to be excluded from satisfaction because of its relatively lesser urgency.

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Nor is it an exception to the law of diminishing marginal utility that an increase in the supply of a good from n to $n + 1$ units can lead to an increase in the utility attached to one unit of this good if such a larger supply, considered and evaluated as a whole, can be employed for the satisfaction of a want deemed more valuable than the value attached to all the satisfaction that could be attained if the units of supply were each employed separately for the various goals that could be achieved by means of one individual unit of such good.[51] However, in such a case, the increase in supply would not be one of supply-units regarded as equally serviceable, because the units simply would no longer be evaluated separately. Rather, in increasing the supply from n to $n + 1$, a different, larger-sized-unit good would be created that would be evaluated as such, and the law of diminishing marginal utility would then apply to this good in the same way as it applied to the smaller-sized good in that the first unit of this good of size $n + 1$ would again be employed for the most urgent use to which a good of this size could be put, the second unit of supply of such sized good would be employed for the second most important goal to be satisfied by such sized good, and so on.

The law of demand then, as grounded in this a priori valid theorem, has never made the unqualified prediction that less of a good will be bought if its price rises. Rather, it states that this will be the case only ceteris paribus i.e., if no increase in the demand for the good in question occurs over time and if the increase in its supply does not effect a different, larger-sized-unit good and, mutatis mutandis, the demand for money does not decrease nor does its smaller supply effect separately evaluated smaller-sized money units.[52] Since it is impossible to have a formula that allows one to predict whether or not such changes occur concurrently with the given rise in price (such changes being dependent on people's future states of knowledge and future knowledge being in principle unpredictable based on constantly operating causes), such a priori knowledge then has a rather limited usefulness for one's business of predicting the economic future. Nevertheless it acts as a logical constraint on predictions in that of all forecasters who equally correctly guess that no such concurrent change will take place, only he who recognizes the law of demand will indeed make a correct prediction, while he whose convictions are at variance with the law will blunder. Such is the logic of economic predictions and the function of praxeological reasoning.

Empiricism recommends the law of demand because it supposedly looks good—yet we can neither see it, nor would it survive empirical testing. Hermeneutics, on the other hand, recommends it because it supposedly sounds good—yet to some it sounds bad. And without some objective, extralinguistic criterion of distinguishing between good or bad, it is impossible to say more in support of the law of demand than somebody said so.

Austrians, as should be clear by now, have no reason to take either the old empiricist fashion or the new hermeneutical one very seriously. Instead,

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they should take more seriously than ever the position of extreme rationalism and of praxeology as espoused above all by the "doctrinaire" Mises, as unfashionable as such a stand might now be.

Notes

1. Princeton, N.J.: Princeton University Press, 1979.

2. This is also the thesis of a book by Paul Feyerabend, *Wissenschaft als Kunst* (Frankfurt/M.: Suhrkamp, 1984).

3. See the interview with McCloskey in the Institute for Humane Studies Newsletter *Institute Scholar,* vol. 6, no. 1 (1986): 7.

4. On this "Apriori of Argumentation," see K.O. Apel, *Transformation der Philosophie,* vol. II (Frankfurt/M.: Suhrkamp, 1973).

5. In connection with the hermeneutical movement, the phrase *intellectual permissiveness* was coined by Henry Veatch in his essay "Deconstruction in Philosophy: Has Rorty Made it the Denouement of Contemporary Analytical Philosophy?" *Review of Metaphysics,* 39, December 1985.

6. McCloskey asks: "[Do we not] need something . . . besides the mere social fact that an argument proved persuasive?" No, he counters, "talk against talk is self-refuting. The person making it [i.e., raising the preceding question] appeals to a social, nonepistemological standard of persuasiveness by the very act of trying to persuade someone that mere persuasion is not enough" (pp. 38–39).

Ironically, however, this argument does *not* prove his point. On the contrary, the argument can be said to be persuasive only because a self-contradictory position is considered to be false, and not regarded as false because it has been agreed upon. Otherwise, if I did not agree, would not the argument have to be considered false?

7. On the inseparable connection between language and action, see esp. Ludwig Wittgenstein, *Philosophische Untersuchungen,* in *Schriften,* vol. I (Frankfurt/M.: Suhrkamp, 1963).

8. On this, see also H. Veatch (note 5), esp. p. 319f.

9. It is by no means an accident, then, that advocates of every conceivable political ideology can be found among the hermeneuticians. The creed goes with libertarianism and anarchism (McCloskey and Feyerabend), with socialism (Ricoeur and Foucault), and with fascism (Heidegger) as well as with, in most cases, middle-of-the-roadism. Gadamer—the special hero of Don Lavoie and the George Mason University hermeneuticians and one of the murkiest "thinkers" of them all, who manages to fill hundreds of pages without saying anything and who rambles endlessly about interpreting without ever actually interpreting any text in an intelligible way (witness his masterpiece, *Wahrheit und Methode,* Tübingen: Mohr, 1960; English transl., 1975)—successfully advanced his career under Nazism, communism, and liberal democracy. On his philosophy and his life as a vivid illustration of the meaning of hermeneutics, see the brilliant essay by Jonathan Barnes, "A Kind of Integrity," *London Review of Books,* November 6, 1986; see also David Gordon, "Hermeneutics vs. Austrian Economics," Occasional Paper (Ludwig von Mises Institute, Washington, D.C., 1986).

10. On the absolutist, a priori foundations of ethics, see Hans-Hermann Hoppe, "From the Economics of Laissez Faire to the Ethics of Libertarianism," in *Man, Economy*

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and Liberty, Llewellyn H. Rockwell and Walter Block, eds., Auburn, Ala.: Ludwig von Mises Institute, 1988; Hoppe, *Eigentum, Anarchie und Staat,* Opladen: Westdeutscher Verlag, 1986. Ethical absolutism as much as methodological absolutism is very much in disrepute. T.W. Hutchison (*The Politics and Philosophy of Economics,* New York: New York Press, 1981, esp. pp. 196–97) goes so far as to smear everyone committed to such a position as a dangerous, potential dictator—revealingly, without ever going to the trouble of explaining what the ethical or methodological principles *are* whose a priori grounding allegedly implies such a threat. Instead, pluralism—ethical and methodological—is what the enlightened person today professes. Only such pluralism, it is said, permits tolerance and freedom. (See as another typical pluralist Bruce Caldwell, *Beyond Positivism,* London: Allen & Unwin, 1982, chapter 13.) Must it be stressed that this doctrine is entirely fallacious? Without a priori foundation, pluralism is itself just another unfounded ideology and there is no reason to adopt it rather than any other one. Only if a priori valid reasons could be given for adopting pluralism could it claim to safeguard tolerance and freedom. A pluralism that would be merely one of plural values would actually be destructive of both. See on this in particular Henry Veatch, *Rational Man: A Modern Interpretation of Aristotelian Ethics* (Bloomington: Indiana University Press, 1962), pp. 37–46. As contrasted to our modern pluralists, Benito Mussolini understood all this quite well. Veatch cites him on p. 41: "From the fact that all ideologies are of equal value . . . the modern relativist infers that everybody has the right to create for himself his own ideology and to attempt to enforce it with all the energy of which he is capable."

11. In defense of the idea of synthetic a priori propositions, see A. Pap, *Semantics and Necessary Truth* (New Haven: Yale University Press, 1958); B. Blanshard, *Reason and Analysis* (LaSalle, Ill.: Open Court, 1964); P. Lorenzen, *Methodisches Denken* (Frankfurt/M.: Suhrkamp 1968); P. Lorenzen, *Normative Logic and Ethics* (Mannheim: Bibliographisches Institut, 1969); F. Kambartel, *Erfahrung und Struktur* (Frankfurt/M.: Suhrkamp, 1968); F. Kambartel and J. Mittelstrass, eds., *Zum normativen Fundament der Wissenschaft* (Frankfurt/M.: Athenaeum, 1973); Ludwig von Mises, *Human Action* (Chicago: Henry Regnery, 1966); Murray N. Rothbard, *Man, Economy, and State* (Los Angeles: Nash, 1971).

12. On the faulty reason for the use of such labels, see note 10. Recently their use has also become increasingly popular among Austrians such as Mario Rizzo and Don Lavoie in order to characterize and distance themselves from the Mises–Rothbard school within the tradition of Austrianism.

13. Ludwig von Mises, *Epistemological Problems of Economics* (New York: New York University Press, 1981); Mises, *Human Action,* (Chicago: Henry Regenery, 1966); Mises, *Theory and History* (Washington, D.C.: Ludwig von Mises Institute, 1985); Mises, *The Ultimate Foundation of Economic Science* (Kansas City: Sheed Andrews and McMeel, 1978); Murray N. Rothbard, *Man, Economy, and State* (Los Angeles: Nash, 1971); Rothbard, *Individualism and the Philosophy of the Social Sciences* (San Francisco: Cato Institute, 1979); Rothbard, "Praxeology: The Methodology of Austrian Economics," in Edwin Dolan, ed., *The Foundations of Modern Austrian Economics* (Kansas City: Sheed & Ward, 1976).

14. London: Macmillan, 1932.

15. Cambridge, England: Cambridge University Press, 1975.

16. Lionel Robbins, just as the earlier Austrians Carl Menger and Eugen von Böhm-Bawerk, admittedly does not use the term *a priori,* but it should be sufficiently

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clear from his arguments as well as his frequent, approving references to Mises (who does) that Robbins actually *means* to provide an a priori justification of the basic propositions and theorems of economics.

The aprioristic character of economic propositions is explicitly stressed also by Frank H. Knight in "What Is Truth in Economics," in *On the History and Method of Economics* (Chicago: University of Chicago Press, 1956).

For those familiar with the tradition of rationalist philosophy, it hardly needs to be shown that the claim of having produced an a priori true proposition does *not* imply a claim of being infallible. No one is, and rationalism has never said anything to the contrary. Rationalism merely argues that the process of validating or falsifying a statement claiming to be true a priori is categorically different from that of validating or falsifying what is commonly referred to as an empirical proposition. However, since McCloskey does seem to think that rationalism assumes infallibility and, hence, that the fact (triumphantly cited on pp. 33–34) that even in as pure a science as mathematics some alleged water-tight arguments have turned out to be inconclusive after all, constitutes proof of a fundamental flaw in rationalism—assuming here in McCloskey's favor that something such as fundamental flaws can exist at all in the absence of any truly objective standard—this point needs to be stressed here. Revisions of mathematical arguments are themselves a priori. They show only that an argument thought previously to be a priori true is not.

17. His description of Marxist economic methodology, on the same page, is not much better.

18. Karl R. Popper, in order to distinguish his falsificationism from the verificationism of the early Vienna Circle, prefers to label his philosophy "critical rationalism." To do so, however, is highly misleading if not deceptive, much like the common U.S.-practice of calling socialists or social democrats "liberals." For in fact, Popper is in complete agreement with the fundamental assumptions of empiricism (see the following discussion in the text) and explicitly rejects the traditional claims of rationalism, i.e. of being able to provide us with a priori true empirical knowledge in general and an objectively founded ethic in particular. See, for example, his "Why Are the Calculi of Logic and Arithmetic Applicable to Reality," in Karl R. Popper, *Conjectures and Refutations* (London: Routledge & Kegan Paul, 1969), where he advances the traditional empiricist thesis that "only if we are ready to accept refutations do we speak about reality" (p. 212) and "refutes" the idea of the rules of logic and arithmetic being laws of reality by pointing out that "if you put 2 + 2 rabbits in a basket, you *may* soon find seven or eight in it" (p. 211). For a correct placement of Popper's philosophy within the general framework of empiricism, see the sovereign discussion by a leading analytical philosopher, W. Stegmueller, *Hauptstroemungen der Gegenwartsphilosophie,* vol. I (Stuttgart: Kroener, 1965), chapters 9–10.

In fact, it is only fair to say that it is Popper who contributed more than anyone else to persuading the scientific community of the modernistic, empiricist-positivist worldview. In particular, it should be emphasized that it was Popper who is responsible for Hayek's and Robbins' increasing deviations from their originally much more Misesian methodological position. See on this Lionel Robbins, *An Autobiography of an Economist* (London: Macmillan, 1976); Friedrich A. Hayek, "The Theory of Complex Phenomena," in Hayek, *Studies in Philosophy, Politics and Economics* (Chicago: University of Chicago Press, 1964); Hayek, "The Pretence of Knowledge," in Hayek,

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New Studies in Philosophy, Politics, Economics and the History of Ideas (Chicago: University of Chicago Press, 1978), esp. p. 31f. See also Hayek's "Einleitung" to Ludwig von Mises, *Erinnerungen* (Stuttgart: Fischer, 1978), and his "Foreword" to Ludwig von Mises, *Socialism* (Indianapolis: Liberty Fund, 1981).

19. Terence W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: MacMillan, 1938); Milton Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953); Mark Blaug, *The Methodology of Economics* (Cambridge, England: Cambridge University Press, 1980).

20. See on this the excellent discussion in Martin Hollis and Edward J. Nell (note 15), "Introduction."

21. Chicago: University of Chicago Press, 1970.

22. See on this also the trenchant observations of Mises, *Human Action,* p. 872f., "Economics and the Universities."

23. The correct nomenclature is "Herr Professor Doktor."

24. See Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, England: Cambridge University Press, 1970). Empiricists such as Blaug (note 19), p. 17ff., argue that Popper actually realized the possibility of "immunizing stratagems" yet "solved" this problem and thus escaped relativism and skepticism. Nothing could be further from the truth. It is correct that Popper has always been aware of the possibility of immunizing one's hypotheses from falsification. (See his *Logik der Forschung,* Tübingen: Mohr, 1969, chapter 4, sections 19, 20.) His answer to such a threat to his falsificationism, however, can hardly be accepted as a solution. For he actually admits that he cannot show such "conventionalism" to be wrong. He simply proposes to overcome it by adopting the methodological convention of not behaving as conventionalists do. Yet how can such *methodolical conventionalism* (i.e., a methodology without epistemological foundation) claim to establish science as a rational enterprise and to stimulate scientific progress? For such an assessment of Popperianism, see A. Wellmer, *Methodologie als Erkenntnistheorie* (Frankfurt/M.: Suhrkamp, 1967). Thus, the preceding classification of Popperianism as relativism and skepticism.

25. See Paul Feyerabend, *Against Method* (London: New Left Books, 1975); Feyerabend, *Science in a Free Society* (London: NLB, 1978).

26. On the complex relation between Feyerabend and Popper, see H.P. Duerr, ed., *Versuchungen. Aufsaetze zur Philosophie Feyerabends,* 2 vols. (Frankfurt/M.: Suhrkamp, 1980).

27. Strictly speaking, such an empirical refutation would not be entirely decisive and other, a priori reasons would be required to bring empiricism down. (On such reasons, see the discussion in the following text.) For empiricists could in turn challenge the validity of one's description of the facts as being indeed those of technological progress. They could, given their own framework, deny that one can know even simple facts, much less complex phenomena such as technological progress, to be so or so, because even the description of something as a fact would ultimately be hypothetical and hence one's alleged empirical refutation could not be considered crucial in any strict sense. See on the hypothetical character of basic propositions Karl Popper, *Logik der Forschung* (Tübingen: Mohr, 1969), chapter V and appendix X. Ironically, the hypothetical character of basic propositions invalidates Popper's claim, central to

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his entire falsificationist philosophy, that an asymmetrical relationship between verification and falsification exists (i.e., that one can never verify a hypothesis, but can falsify it). See on this A. Pap, *Analytische Erkenntnistherie* (Wien, 1955).

28. See also Juergen Habermas, Der Universalitaetsanspruch der Hermeneutik," in K.O. Apel et al., *Hermeneutik und Ideologierkritik* (Frankfurt/M.: Suhrkamp, 1976), esp. pp. 129–31.

29. See Hans-Hermann Hoppe, *Handeln und Erkennen* (Bern: Lang, 1976).

30. See on this W. Stegmueller, *Hauptstroemungen der Gegenwartsphilosophie,* vol. II (Stuttgart: Kroener, 1975), chapter 5, esp. pp. 523ff.

31. See on this also Juergen Habermas, *Erkenntnis und Interesse* (Frankfurt/M.: Suhrkamp, 1968), esp. chapter II, sections 5–6; and K.O. Apel, *Die Erkaeren: Verstehen Kontroverse in Transzendental-pragmatischer Sicht* (Frankfurt/M.: Suhrkamp, 1979), esp. p. 284.

32. See on this also K.O. Apel, "Die Entfaltung der Sprachanalytischen Philosophie und das Problem der Geisteswissenschaften," in Apel, *Transformation der Philosophie,* vol. II (Frankfurt/M.: Suhrkamp, 1973); Apel (note 31).

33. See on this also Hans-Hermann Hoppe (note 29), chapter 3 and esp. pp. 62–65; also Immanuel Kant, *Kritik der reinen Vernunft,* in Kant, *Werke,* vol. II, W. Weischedel, ed., (Wiesbaden: Insel, 1956), esp. p. 226ff.

34. It is worth emphasizing here that these remarks on the skeptical, relativistic conclusions of empiricism regarding the possibility of prediction also fully apply to Popperianism. Popper, with great self-assurance, claims to have solved—through adopting his falsificationist methodology—the Humean problem of induction and thereby to have reestablished science as a rational enterprise. (See in particular Karl R. Popper, *Objective Knowledge,* Oxford, England: Oxford University Press, 1972, p. 85ff.) Alas, this is simply an illusion. For how can it be possible to relate two or more observational experiences, even if they concern the relations between things that are perceived to be the same or similar, as *falsifying* (or *confirming*) each other, rather than merely neutrally record them as one experience here and one experience there, one repetitive of another or not, and leaving it at that (i.e., regarding them as logically incommensurable) unless one presupposed the existence of time-invariantly operating causes? Only if the existence of such time-invariantly operating causes could be assumed would there by any logically compelling reason to regard them as commensurable and as falsifying or confirming each other. However, Popper, like all empiricists, denies that any such assumption can be given an a priori defense (there are for him no such things as a priori true propositions about reality such as the causality principle would have to be) and is itself merely hypothetical. Yet clearly, if the possibility of constantly operating causes *as such* is only a hypothetical one, then it can hardly be claimed, as Popper does, that any particular predictive hypothesis could ever be falsified or confirmed. For then the falsification (or confirmation) would have to be considered a hypothetical one: any predictive hypothesis would only undergo tests whose status as tests were themselves hypothetical. And hence one would be right back in the muddy midst of skepticism. Only if the causality principle as such could be unconditionally established as true, could any particular causal hypothesis ever be testable, and the outcome of a test provide rational grounds for deciding whether or not to uphold a given hypothesis.

35. See on this (Kantian) idea F. Kambartel, *Erfahrung und Struktur* (note 11), chapter 3, esp. pp. 122f, 127, 144; Hans-Hermann Hoppe (note 29), chapter 4, esp. p. 98.

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36. See on this Ludwig von Mises, *Human Action* (note 13), chapter I.5; Carl Menger, *Grundsaetze der Volkswirtschaftslehre* (Vienna: Braumueller, 1871), pp. 3, 7ff.

37. Though quite frequently mentioned as an empirical counterexample, it should be noted that quantum physics, or more precisely the indeterminacy or Heisenberg principle of quantum physics, correctly interpreted, is in accordance with this. What has been previously said does not preclude—and this is precisely the situation in quantum physics—that in order to experimentally produce a result, two or more measurement acts must be performed and that because any two separate actions can only be performed sequentially, the performance of the latter act of measurement might change the results of the former one, so that if this proves to be unavoidable, the results in question can only be predicted statistically and a deterministic explanation proves impossible. But even here each separate measurement act presupposes the validity of the constancy principle—otherwise, neither of them would have been performed; and the sequence of acts, too, presupposes constantly operating causes as it would otherwise be simply impossible to repeat two experiments in the field of quantum physics and state this to be the case. Moreover, the experience of quantum physics is in exact line with the preceding conclusion regarding the characteristic of causality as an action-produced phenomenon and as a necessary (known to be valid a priori) feature of reality. If causes can indeed only be measured and identified sequentially, by means of actions that have repercussions for each other, then they can, in principle, only be causes whose constant operation is of a probabilistic kind—and this, to be sure, can again be known to be true a priori. Quantum physics then only reveals that cases such as this are not merely conceivable, but do in fact exist. See on this F. Kambartel, *Erfahrung und Struktur* (note 11), p. 138ff.; also P. Mittelstaedt, *Philosophische Probleme der odernen Physik* (Mannheim: Bibliographisches Institut, 1968).

38. *Human Action* (note 13), pp. 870–71.

39. Mises correctly emphasizes that the decisive argument against causal predictions in economics must be the absence of "constant relations" in the field of human knowledge and actions. See, for instance, *Human Action* (note 13), p. 55f.

40. See on the following Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (Opladen: Westdeutscher Verlag, 1983); Hoppe, "Is Research Based on Causal Scientific Principles Possible in the Social Sciences," *Ratio,* XXV, no. 1, 1983.

41. On this, see note 34.

42. Interestingly, this proof has been first formulated by Popper in the preface of his *The Poverty of Historicism* (London: Routledge & Kegan Paul, 1957). However, Popper failed to realize that such proof actually invalidates the idea of a methodological monism and demonstrates the inapplicability of his falsificationist philosophy in the field of human action and knowledge. See on this Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (note 40), pp. 44–49; K.O. Apel, *Die Erklaeren: Verstehen Kontroverse* (note 31), pp. 44–46, note 19.

43. See on this H. Albert, *Traktat ueber kritische Vernunft* (Tübingen: Mohr, 1969), esp. chapter 5.V, VI.

44. Mises writes:

> The essence of logical positivism is to deny the cognitive value of a priori knowledge by pointing out that all a priori propositions are merely analytic. They do not provide new information, but are merely verbal and tautological. . . . Only experience can lead

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> to synthetic propositions. There is an obvious objection against this doctrine, viz., that this proposition is in itself a—as the present writer thinks, false—synthetic a priori proposition, for it can manifestly not be established by experience. (*The Ultimate Foundation of Economic Science* [note 13], p. 5.)

It is remarkable to notice how utterly helplessly empiricists react to such arguments establishing the case for synthetic a priori propositions. Witness, for instance, Mark Blaug, *The Methodology of Economics* (note 19), pp. 91–93, where he engages in an all-out smear attack on Mises ("Mises' . . . later writings on the foundations of economic science are so cranky and idiosyncratic that we can only wonder that they have been taken seriously by anyone," p. 93) without presenting a single argument and without noticing how strangely his self-assuredness and the apodicticity with which he presents his antiapriorist methodological pronouncements contrast with his very own professed falsificationism. The same discrepancy between, on the one hand, a complete lack of argument and, on the other, apodictic arrogance, also marks his "discussion" of Hollis and Nell's *Rational Economic Man* (note 15) on pp. 123–26.

45. Empiricists, of course, would brand such definitions as tautological. Yet it should be perfectly clear that the preceding definition of action is of a categorically different nature than a definition such as "*bachelor* meaning "unmarried man." Whereas the latter is indeed a completely arbitrary verbal stipulation, the propositions defining action are most definitely not. In fact, while one can define anything as one pleases, one cannot help but make the conceptual distinctions between goals and means and so on as "defining something by something" would itself be an action. It is thus contradictory to deny, as empiricism-positivism does, the existence of "real definitions." Hollis and Nell (note 15) observe "Honest definitions are, from an empiricist point of view, of two sorts, lexical and stipulative" (p. 177). But

> When it comes to justifying [this] view, we are presumably being offered a definition of *definition*. Whichever category of definition the definition falls in, we need not accept it as of any epistemological worth. Indeed, it would not be even a possible epistemological thesis, unless it were neither lexical nor stipulative. The view [then] is both inconvenient and self-refuting. A contrary opinion with a long pedigree is that there are "real" definitions, which capture the essence of the thing defined. (p. 178)

See also B. Blanshard (note 11), p. 268f.

46. Hollis and Nell (note 15, p. 243) contend that not "action" but "reproduction of the economic system" is the primary concept on which economics, conceived of as an a priori science, rests. Noticing this disagreement among apriorists has led Caldwell (note 10, p. 131ff.) to the curious conclusion that something must be wrong with apriorism and to then advocate a do-not-commit-yourself-to-anything pluralism. (See note 10.) Yet such reasoning is about as conclusive (or, rather, inconclusive) as inferring from the fact that disagreements among people regarding the validity of certain empirical propositions exist, that no empirical facts exist and hence no empirical science is possible. Indeed, Caldwell's conclusion is even more curious, given the fact that in the dispute at hand, the solution is as clear as daylight: Whatever an economic "system" might be, it can certainly not exist or be able to reproduce itself without acting people. Moreover, to say that "reproduction of the system" is the primary concept for economic analysis is plainly contradictory—unless it were simply synonymous with saying that action is such a concept—because saying so would in fact presuppose an actor saying it.

47. On the empiricist position regarding the law of demand, see Mark Blaug (note 19), chapter 6.

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48. Moreover, why would the argument not also go the other way? If, empirically, the law of demand does not seem to work for some goods, why would not analogy lead us to question it for those in which it does? (I owe this argument to David Gordon.)

49. See on this Ludwig von Mises, *Human Action* (note 13), p. 124.

50. Robert Nozick ("On Austrian Methodology," in *Synthese,* 36, 1977) believes Austrians to be inconsistent in (1) claiming that actions invariably show preference (and never indifference) and (2) employing the idea of "*homo*geneity" and "*equal* serviceability" of goods in their formulation of the law of marginal utility (p. 37ff.). However, such a charge would only be correct if "preference" and "indifference" were both considered categories of the same type. This has been correctly pointed out by Walter Block ("On Robert Nozick's 'On Austrian Methodology'," *Inquiry,* 23, 1980), who insists that "indifference" is not, unlike "preference," a *praxeological* category. Yet his classification of indifference as a "psychological category" instead (p. 424) is also incorrect. In fact, "sameness" is an *epistemological* category: humans are knowers *and* actors; they only act because they know, and they only know because they act. That something is the same (or different) than something else we know qua actors who *know.* (Indeed, "sameness' is a universal epistemological category in that one could not even say anything, for instance about actions, without the notion of something being an instance of some particular *type* of thing.) That something that is known to be the same can never actually be treated with indifference we know qua knowers who *act.* The law of diminishing marginal utility then is a law regarding knowers who *act.*

51. See Ludwig von Mises, *Human Action* (note 13), p. 125; M.N. Rothbard, *Man, Economy, and State* (note 13), p. 268ff.

52. Empiricists will complain that such a formulation of the law will make it tautological and unfalsifiable. Both classifications are false. Clearly, the discovery of a new, more highly valued use for, for instance, a unit of a given good, i.e. the event "increase in demand," and the event "a higher price is paid for it" are two conceptually distinct events, and to logically relate such events then is a categorically different thing than to stipulate that "*bachelor* means 'unmarried man'." (See also note 45.) That the use of ceteris paribus clauses in economics implies an immunization strategy, on the other hand, would be true only if economic propositions were indeed concerned with contingent empirical causal laws. In the natural sciences, where laws do have this status, such complaint would be appropriate—yet there, interestingly enough, one hardly ever finds ceteris paribus clauses. In the natural sciences, predictive hypotheses of the form "If . . . then" are in fact treated as applicable whenever the if-condition is given, no matter what else is or is not the case. And it is only because this is done that such hypotheses can be validated at all. (There is only one way of testing hypotheses about contingent empirical causal relations: in and through factual applications.) If, contrary to this, one were to demand that in order to apply a hypothesis or to repeat its application, a *full* description of the world at the moment of application be given, or that *everything* be the same in the second application as in the first (beyond the sameness of the condition explicitly stated in the if-clause), the hypothesis would become inapplicable and thus empty for the practical reason that such a demand would literally involve describing all of the universe, and for the theoretical reason that no one at any point in time could possibly know what all the variables are that make up this universe (as this question remains open to new discoveries).

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The situation is entirely different in economics, and it is curious indeed that this should not have been realized—given the facts that the use of ceteris paribus clauses in the empirical sciences would render such sciences futile and that such clauses are nonetheless constantly employed in economics. Why, then, not give serious consideration to the idea that economics might be an altogether different science? Indeed, as we have seen, this is the case. Economic propositions can be validated independently of any factual application as implied (or not implied) in the incontestable axiom of action plus certain situations and situational changes described in terms of the categories of action. Yet then ceteris paribus clauses are completely harmless. In fact, their use simply serves to remind us that the deduced consequences only follow if the situation is indeed as described (and not a logically praxeologically different one), and that it is impossible in all actual applications of economic theorems (i.e., whenever the situation analyzed can be identified as real) to hold the ceteris experimentally constant (as the "holding constant" then can, in principle, only be done logically, by means of thought-experimentation. See on this also Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (note 40), p. 78–81.

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A Look at *Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on his Eightieth Birthday*

Walter Block

In his preface to this volume, Israel Kirzner "draw[s] attention to the altogether special contributions made by Professor Lachmann to the creation and growth of the Austrian Economics Program at New York University" and to his "crucially important part in the remarkable revival of interest in Austrian economics" (p. viii)[1] in the profession as a whole. This is altogether correct. Lachmann's long and distinguished written contribution to the praxeological school and especially his arrival on the scene at New York University (NYU) were absolutely pivotal; in the absence of the role he played, there is a great question as to whether or not there would have been any Austrian revival at all.[2] Professor Lachmann functioned as a gadfly. With his iconoclastic views and his continual criticism of all received Austrian axioms and doctrines, he served as a constant challenge, spur, and inspiration—not only to his colleagues and students at NYU, but also to all those such as myself who had the good fortune to come into contact with him.

This is why it is all the more highly unfortunate that a book committed at least in part to the renaissance of the Austrian school makes, with just a few honorable exceptions, so little contribution to that estimable goal. While the Lachmann *Festschrift* is indeed partially dedicated to the recent rise of the praxeological school of thought, it is devoted to an even greater degree to the life and work of Professor Lachmann himself. As a result, it is not so much a praxeological book as a Lachmannian one. Kirzner, in his preface, says as much: "Despite Ludwig Lachmann's participation in this renaissance of Austrian economics, the present volume is not a collection of Austrian papers" (p. viii).

The author wishes to express appreciation for comments and criticisms of an earlier draft to Jeffry Tucker, David Gordon, Murray Rothbard, Michael Edelstein, and two anonymous referees. They, of course, are not responsible for any errors that remain.

Review of *Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on his Eightieth Birthday,* Israel Kirzner, ed. (New York: New York University Press, 1986).

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However, the present volume has been widely hailed as the hermeneutical response to Austrianism, as "the best thing written since *Human Action*," and as the ultrasubjectivist new paradigm or model, the path to be followed by the "new" Austrian movement. This, as we shall see, is unwarranted. As well, it is important to put to rest even the more modest view that "This book can be recommended to serious students wishing to know what Austrian economists are thinking about today."[3]

On the contrary, this book is a collection of highly heterogeneous essays, the overwhelming majority of which have nothing at all to do with praxeology. Some are written by mainstream economists in almost complete ignorance of the Austrian tradition; as a result, their views are at best irrelevant to this school of thought. Included under this rubric are the contributions of Boland, Hicks, Langlois (who discusses game theory), Lewin, and Torr (who grapples with the Rational Expectations and Ricardian schools, but treats neither from an Austrian perspective). The institutionalists are represented by Perlman, who attempts no less a task than to equate Austrianism and the German Historical school, against which Menger and Mises waged a decades-long intellectual battle. The hermeneuticians (Ebeling and Lavoie) undertake no less in behalf of their philosophy with equal success. Then there are those who uphold the post-Keynesian system (Egger and Kregel), without, unfortunately, deigning to reply to the Austrian critique. There are as well the nihilists (Fehl, High, and Strydom), whose goal is to deny the validity of equilibrating forces. Fehl, for example, goes so far as to claim that not only is the market order impossible, it is undesirable too (p. 79).[4] There are also the obscurantists (Addleson and Shackle) about whose essays it is impossible to say anything coherent.

Quite apart from the fact that most of the book is devoted to non-Austrian viewpoints, there is also the shortcoming that many of the tracts are simply fallacious, even on an elementary level. Egger, for example, appears to call into question the notions of time preference, degrees of roundaboutness, and structure of production, likening them to "primitive concepts which have brought Austrian theories of capital and the business cycle much scorn" (p. 64). He does so without defending this position against the mainstream Austrian analysis, which sees these concepts as a basic building block of praxeology. Boland, a methodologist, appears not to recognize any incompatibility between Walrasianism and market process (32–33). Strydom conflates subjectivism and market process. And Rizzo and O'Driscoll reprise their book (1985) without taking into account Baird's many fundamental and telling criticisms.[5]

Out of a total of twenty-three papers, only six are both relevant to Austrian economics and free from multitudinous and egregious errors: those written by Boehm, Garrison, Hutchison, Kirzner, Mittermaier, and White. These authors are the honorable exceptions; they make real and important contributions to praxeology. Their work may truly be taken as indicative of what Austrian economists are thinking about today.

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Space permits a detailed analysis of only a small proportion of this material, and it is to this task that I now turn. I begin with two articles that are highly critical of Austrian economics.

High

Jack High's "Equilibration and Disequilibrium in the Market Process" is an attempt to "defend Lachmann's thesis that the marketplace is a disequilibrating as well as an equilibrating process" (p. 112). The article is an interesting one, but cannot successfully maintain that thesis.[6]

Professor High gives an accurate and insightful account of the evenly rotating economy (ERE), as utilized by Mises, Rothbard, and Kirzner. He asks: "If the market is always moving toward an evenly rotating state, why doesn't it ever get there?" and cites the traditional Austrian answer: because "the end state is always moving," due to exogenous changes in tastes, technology, and resources (p. 113). But our author rejects this, on the ground that it unnecessarily and artificially limits the scope of economics. On the contrary, asserts High, these phenomena are endogenous: "We know that an open market for ideas will lead people to change their value scales, that the prospect of profits will induce entrepreneurs to seek out new technologies and uncover new resources" (p. 113). Further, to take these changes as exogenous to the market "is to take facts, the knowledge of which should be explained, as given" (p. 114), something Hayek forcefully and eloquently warned against.

The problem with this line of argument, however resourceful and inventive, is that it multiplies the scope of economics way beyond all reasonable bounds. To be sure, changes in tastes, technology, and resources "should be explained," but why must this be done by economists? At present, knowledge of consumer tastes is the domain of psychologists, physiologists, or market researchers; for analysis of technology, we defer to the physicist, chemist, or engineer; nor does the mere armchair economist delve into the arcane mysteries of resource discoveries and development, wisely leaving these matters to the geologist. I am as receptive as the next dismal scientist to the expansion of our turf (see Becker [1971, 1976]; Bernholz and Radnitzky [1986], but this idea of High's would appear to be imperialistic to a degree so far undreamt of.

Nor is this savaging of the intellectual division of labor the only drawback in the scheme proposed by Professor High; it also has a serious logical flaw. He cites Lachmann as stating: "To speak here of 'random shocks' would be to profess ignorance where we have knowledge" (p. 113).[7] Of course we have knowledge of tastes, technology, and resources. But this is only as *laymen,* not professionals. Using this as a criterion, we know *something* about *everything.*[8] It is therefore not merely the case that tastes, technology, and resources happen to be exogenous to economics, as per the views of those stick-in-the-mud

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Austrians Mises, Rothbard, and Kirzner. High is logically tied to the rather amazing claim that there simply could not be anything at all exogenous to economics! According to his reasoning, even a flood, a tornado, a hurricane, or a volcano would not be exogenous to economics. This would be to profess ignorance where we have knowledge. For do we not know something about floods, tornados, hurricanes, and volcanos? We most certainly do. In addition to the knowledge that meteorologists can provide about these phenomena, we know that they exist.

As part of his overall thesis, Professor High next makes the claim that "the same active mental processes which are taken to adjust to change once it has occurred [entrepreneurship], will also originate change" (p. 115). In this, High is influenced by Schumpeter's views of entrepreneurship, namely that the profit-seeking businessman will destabilize the economy. Unfortunately, however, he nowhere mentions Kirzner's utter and complete evisceration of Schumpeter on this point. This is indeed puzzling, since High's footnote 7 cites this literature, so he can hardly be unaware of it. All that could be done in refutation of this viewpoint would be to recite the brilliant Kirznerian analysis. Since this is readily available, and it is beyond the ability of the present author to improve on it, I shall content myself with referring the reader to it. I must note, however, that it is highly disappointing that the author of the chapter under discussion chose to defend Schumpeter against Kirzner without specifically addressing the points made by the latter against the former.

High places great emphasis on the fact that "money, monetary calculation, business firms, and advertising emerge and persist in the market." This, in his view, "belies the claim that the market is a strictly equilibrating process" (p. 117). But such occurrences are readily explained by the existence of exogenous forces that continually keep the market in a state of flux and render our knowledge obsolete. This is why we still have need for these institutions that promote and economize on information—despite the fact that the market is strictly an equilibrating process.

Surprisingly, after fighting mightily against equilibrating forces as a demonstration "that order will emerge from decentralized decision making" (p. 117) and as an organizing tool for economists, Professor High seizes upon profit-seeking behavior for this very purpose. Our author gives an insightful and eloquent account of how the entrepreneurial search for wealth helps in the development of the complex economy, and he even contributes to a determination of the extent of the division of labor. But this is still highly problematic, at least on the theory he propounds, because it is difficult to imagine a more equilibrating phenomenon than the desire to capture profits.

Further, we could use the very arguments employed by High in criticizing the Austrian view, in opposition to his proposal that profit seeking is disorderly. For example, why is it that profits have not by now been completely competed away? High could not reply that this is due to exogenous shocks to the system,

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for he is committed to the view that there are no such things. Moreover, what about the destabilizing (read: disorderly) aspects of profit seeking? Why has Schumpeter been so suddenly and unceremoniously been shoved out the back door?

I have interpreted High as attempting to defend the Lachmannian "kaleidic" hypotheses that there is no market tendency toward equilibrium. According to an alternative view, the implicit thesis underlying this chapter is that economics should be radically redefined. Austrianism should be limited to a study of the evolution of institutions. All notions of equilibrium are erroneous and should be eschewed, to the extent that they imply an end state. Instead, economics should focus on describing patterns of evolution, with the Misesian monetary theory serving as a model.

If this interpretation is correct, there are several additional difficulties with High's views. So radical a restructuring would make praxeology unrecognizable; even assuming the notion to have merit, why not define a new discipline (call it Highism or Lachmannianism) separate and distinct from Austrianism? There is also a failure to make a sharp enough distinction between Walrasian general equilibrium, which is inapplicable to reality, and a tendency toward equilibrium, which is not. Most important, we must underscore the Misesian point that all action is inherently aimed at achieving equilibrium.[9] The economic actor purposefully tries to remove a felt uneasiness, not to increase it. As well, Mises' monetary theory, just like the rest of his system, was praxeological, not evolutionary.

High's article is a welcome one in that it thoughtfully tries to show that disequilibrium is an integral part of the market. If the Austrian edifice is to be built sturdily, it will have to withstand such onslaughts as directed to it by this author. Thanks to him, it is now in a better position to do just that.

Lavoie

Professor Don Lavoie sets himself the herculean task of showing that Mises was really a closet hermeneutician. The first arrow in his "Euclideanism versus Hermeneutics: A Reinterpretation of Misesian Apriorism" is the claim that there is no strict dichotomization, in Mises, between theory and history.[10] And Lavoie's evidence for this rather dubious point? The fact that strict dichotomization between one thing and another is not equivalent to their isolation. He illustrates this by showing, quite correctly, that theory and history "are absolutely necessary for one another" (pp. 193–94).

But this will not do. First of all, this is a straw-man argument. No one has ever claimed, least of all Mises, that the absolute dichotomy between theory and history implies that they must always and ever be kept completely apart. On the countrary, Mises (1969) should serve as strong evidence that the two can and indeed have been brought together. Second, let us argue from analogy:

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in like manner, we could say, there is a strict logical dichotomy between normative and positive economics, and a biological distinction to be drawn between male and female. Yet it would be ludicrous to demolish the argument that therefore each of the members of these two pairs ought to be held in complete isolation from one another—if for no other reason than no one has ever made any such bizarre claim. We must therefore reject this view of Lavoie's and insist that for Mises, not only does there exist a deep chasm between theory and history, but that the two concepts, no matter how different, are still complementary.

The next shot across the bow of the good ship Mises is Lavoie's "analysis" of praxeology and apriorism. Consider his charge concerning "the antagonism to empirical work" (p. 195). The problem, here, is an equivocation. Given that for Mises, economics is a logically deductive system, akin to mathematics, no empirical finding, in and of itself, could possibly overturn a praxeological conclusion. Naturally, under these circumstances, a "hostility" to empirical work would appear to arise. But this is true only if the regression equations are interpreted as *testing* an a priori law. On the contrary, there was not an aversion to empirical work per se, as shown by Mises' warm regard for economic history as providing an illustration, not a test, for the theoretical findings of economics.

Lavoie also misinterprets, indeed confuses, "dogmatism and rigidity . . . and [a] confident air of completeness and apodictic certainty" (p. 195) with the Misesian methodology of praxeology. The former is a psychological state, the latter a philosophical category, and no direct translation from one to the other is justified. That is, one can be a deductive economist and yet hold all one's findings in a timid, tentative manner. And the same goes for mathematicians, who also practice an apodictic calling. Alternatively, one can be dedicated to positivism, falsifiability, and empirical testing, while yet being personally obnoxious, dogmatic, rigid, and deaf to all criticism. No facile deduction from the economic methodology employed by a scholar to his psychological state is thus justified.

Lavoie then criticizes Mises for holding the view that "No historical account can ever cause us to go back and reconsider our a priori theory, thus suggesting that theories are somehow epistemically privileged and safely dichotomized from history. This view makes economics seem too different from the natural sciences" (p. 196). Again, there is a confusion between economics and psychology. There are numerous phenomena that cause people to go back and reconsider. It could be a sunset, a Mozart opera, or an intellectual unease that just comes upon a person; it all depends upon the individual in question. In some cases, of course, there is nothing that can lead a person to reconsider a pet theory. But this issue must be sharply distinguished from a subtly different one. If the theory seems to be out of step with the facts, must we necessarily renounce the theory? Suppose, for example, that an empirical finding determines that an increase in the minimum wage level led to a *decrease* in the teen unemployment rate, or that the imposition of a strict rent-control ordinance

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called forth a vast building spree of rental housing. Would economists necessarily be forced to reject their typical views of such legislation?[11] To ask this question is to answer it. The point is, there is a vast distinction to be drawn between the natural sciences and the human sciences. It is only in the latter case that motives and purposes are comprehensible to us.

The George Mason University professor then goes on to designate as "hermeneutic" the insight that economic explanation must be grounded in individual purposes (p. 196). If we allow him to get away with this bit of definitional legerdemain, we must indeed conclude with him that Mises was a hermeneutician, because if there was anyone who insisted that praxeology be based on the purposes of the individual, it was Mises.

Perhaps the most interesting part of the Lavoie contribution is his analysis of the textual evidence. He starts out well, citing Mises in several of his more "Euclidean" utterances. His first criticism is that Mises is a "bit free" (p. 200) in attributing certainty and incontestability to his pronouncements. Two responses are in order here. First, there is a failure to distinguish the claim that economics is an apodictically certain enterprise from the one that any particular economist, such as Mises, is apodictically certain. In the enumerations of Mises' words in his text, our author indicates only the former declaration, not the latter. Second, if Lavoie wishes to attack Mises for expressions of excessive certainty, it is incumbent upon him not merely to specify instances where he does indeed express certainty, but where these expressions are in fact excessive. Namely, he has to show him to be *wrong*, in at least one instance. For example, let us suppose that Mises, as mathematician, had claimed not only that "2 + 2 = 4" was apodictically certain in mathematics, but that he, Mises, was himself indubitably certain about the truth of this equation. Were Lavoie to criticize Mises for this, on the grounds that he was *excessively* certain about this, it would be insufficient to merely charge the use of certainty; he would have to show it to be *false*. Otherwise, although there would indeed be certainty, it would not be shown to be excessive.

Next, Lavoie objects to the exclusivity that Mises claims for the focus of praxeology on human purposiveness (p. 201). But this does not "suggest an insulation from criticism." Were this true, then Mises, or his followers, when presented with a criticism, would merely have dismissed it, without giving reasons. Interestingly enough, Lavoie is unable to substantiate his charge of "insularity," although he is fully familiar with the Austrian literature. Nor does he deign to discuss or even refer us to a valid economics that does *not* ground itself on individual purpose. Neoclassical macroeconomics, for instance, has virtually entirely banned consideration of individual purpose from its realms. Is Lavoie claiming that this mode of thought can make a contribution to our knowledge of economic reality?

At last we arrive at the nub of the Lavoie thesis: Mises as hermeneutician. Our author starts off with a quote from Mises where he states, quite reasonably,

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that "Reasoning and scientific inquiry can never bring full ease of mind, apodictic certainty, and perfect cognition of all things" (p. 203). After that, the analysis goes sharply downhill. Lavoie first sees in this modest statement a contradiction with previously cited "Euclidean" avowals of Mises. But he again fails to contend with the distinction between psychology and economics. One can, as Mises does, adopt a modest attitude toward one's own abilities, even in behalf of all mankind, and still noncontradictorily demonstrate the apodictic nature of an intellectual discipline such as geometry or economics.

Second, Lavoie's treatment is marred by his continued and unabated gratuitous attacks on poor old Euclid, without benefit of citation. From whence comes "Euclidean uncriticizability" or "close it off in Euclidean fashion from rational criticism" (p. 203)? When and where did Euclid (or Mises, for that matter) claim that all criticism of their views was per se illegitimate?

Third, the "hermeneutic a priori" would appear to differ from its Euclidean counterpart only in that the latter, but not the former, is "immune to criticism" (p. 204). Certainly, part of the Misesian defense for the a priori nature of the praxeological axioms was that those who denied this would "contradict themselves in practice by arguing their behavioralistic case to other 'minds' in the scientific community" (p. 204).

Fourth, his case is marred by a needless bow to pragmatism. Contrary to Lavoie, the assumption of intersubjectivity *is* based on firm philosophical foundations. Were it not, what would it mean to assert that "this procedure works in everyday life" (p. 206)? Without the philosophical grounding, we would have no reason to reject the hypothesis that all other people are just cleverly constructed robots.

As for the remainder of the chapter, it consists of Lavoie citing numerous modest expressions by Mises and, by Lavoie's torturing them, teasing out a hermeneutical implication, solely on the grounds of the constraint of these statements.[12] He only succeeds in showing, contrary to his previous assertions, that Mises was a careful, moderate, and cautious economist.[13]

Boehm

"Time and Equilibrium: Hayek's Notion of Intertemporal Equilibrium Reconsidered" by Stephan Boehm is a welcome addition to the Austrian literature in general and to our knowledge of capital theory, interest rates, time, and their complex relationships. It also makes a significant contribution to history of economic thought, serving as a sort of secondary source material for, and clarifier of, Hayek's very turgid *The Pure Theory of Capital* (1941, 1975).

The author distinguishes authoritatively between Hayek's notion of equilibrium (intertemporal mutual plan compatibility) and the more mechanistic, static, and a temporal version still popular in the profession (pp. 20–21).

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He properly credits Hayek for strenuously resisting the widely accepted notion that capital is merely a "homogeneous substance" (p. 22) or an "amorphous mass" (p. 23) and instead insisting upon its heterogeneity. He criticizes the Arrow-Debreu equilibrium model from a Hayekian perspective, showing that the concept of "movement toward" an equilibirum makes no sense in the former case (p. 24). Most important, he places great emphasis on the importance of subjectivism in capital theory, on Hayek's success, and on Arrow-Debreu's failure to distinguish between the perspective of the human actor and that of the omniscient economist-observer (p. 25).

Further, Boehm is correct in focusing on something of an inconsistency that crops up in *Prices and Production* (1932). On the one hand, this book posits what is in effect an equilibrium model of the structure of production; on the other hand, Hayek attempts to show the destabilizing effects of government monetary policy. The difficulty is that, strictly speaking, the existence of money is incompatible with the certainty that underlies equilibrium. However, it is easy to make too much of this criticism. There is somewhat of a lack of appreciation that the evenly rotating economy is used by Hayek merely as a heuristic device. One searches in vain in *Prices and Production* for the Rational Expectations type of confusion that would ascribe to the real world the existence of actual equilibrium.

As well, there is one other minor criticism that can be leveled against this chapter. Boehm mentions without demur that there might possibly be "no such thing as *the* rate of return or interest since there are as many rates as there are commodities" (p. 19). But why is there no profit-and-loss–inspired tendency for all these different rates to come together?

But these are minor quibbles with a superior product, which will repay, with great interest, the effort of reading it.

Garrison

Roger Garrison's contribution to the Festschrift, "From Lachmann to Lucas: On Institutions, Expectations, and Equilibrating Tendencies," is nothing short of magnificent. By creating a "tendency toward equilibrium spectrum" and then placing on it Lachmann ("never"), Mises-Hayek ("sometimes"), and Lucas ("always"), Professor Garrison does more to elucidate the views of the Austrian, the Rational Expectations, and the "kaleidic" schools of thought on the equilibrating tendencies in an economy than an essay of ten thousand words or more could have done. Through this astute and innovative feat, moreover, the author of this chapter is able to once again establish praxeology as the moderate and reasonable view that takes on an intermediate position between two extremist beliefs that might otherwise have appeared more attractive than they are. (See Garrison [1982].)

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Garrison uses Lachmann's concern with future expectations to cast doubt on Lucas's assertion that the economy must always and ever be in equilibrium. He mobilizes the Mises-Hayek insight that on the free market, those who are better able to anticipate consumer demands will tend to have command over more and more resources and thus will be able to cast a disproportionate impetus toward equilibrium. This undercuts the extreme Lachmannian skepticism that there can be even a tendency toward equilibration.

Further, with this spectrum device, the Auburn University professor can focus attention on the crucially important role played by institutions. The Hayekian criticism of Keynes is that there is not enough disaggregation in this system to allow for the equilibrating role of entrepreneurial success. But this can only occur, shows Garrison, in a marketplace where businessmen can reap the reward of their superior insight. Paradoxically, or perhaps not so paradoxically, Keynesian-inspired government "stabilization" measures can actually retard movements toward equilibrium. Says Garrison: "They nullify the market forces that give rise to equilibrating tendencies, thus causing the economy to perform in the very way that Keynes envisioned it" (p. 99).

Notwithstanding the numerous brilliant insights gracing this essay, there are two minor points upon which criticism can be offered. First, in the course of his quite proper formulation of the Hayekian theory of capital, Garrison asserts that "Successful investment over time requires that the investor's decisions be consistent *both with the subsequent decisions of other investors* and with the ultimate demands of the consumers" (p. 96, emphasis added).

If we interpret this unsympathetically, counterexamples are easy to come by. For instance, if one entrepreneur and he alone correctly forecasts consumer interest in, say, a hula hoop, then he will make more profit, not less, than if he is in accord with the decisions of other investors. To be sure, there is a sense in which it is true that not only must the thriving entrepreneur be attuned to the consumer, he must also act in concert with his fellow businessmen. For example, even if he correctly anticipates future demand, but no one else does, he may be so far out of step, even though correct in some ultimate sense, that the complementary factors of production may not be available. Sometimes this is called "being too far ahead of one's time." But even in this case it may not be necessary to be "consistent with the subsequent decisions of other investors." For it may be possible, if he is rich enough, for him to finance the production of not only the item in question, but also the complementary factors as well. In such a case, his earnings would be even higher. It is only if this is beyond the capability of one single entrepreneur that Garrison's stricture will be correct.

Second, Professor Garrison maintains the position that "Investors who are able successfully to complete their projects gain command over greater quantities of resources. In turn, the subsequent decisions of these successful investors have increased weight in *determining the market rate of interest*" (p. 98, emphasis added). The difficulty here is that this would appear to be contrary to the

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Austrian pure time preference theory of interest-rate determination. Since our author is himself on record as supporting this view (see Garrison, 1979; also Rothbard, 1962, chapter 6; Mises, 1966, pp. 479–523), we need not take time here to defend it. Of course, one might read the passage as claiming that since these investors will now have more money *qua consumer*, their own time preference will now be given a greater weight in interest-rate determination. If this were true, it would be unexceptionable. However, in the second sentence after the one just cited, Garrison quite clearly avers that it is "subsequent *investment* decisions by these investors" (p. 98, emphasis added) that constitute the operational factor involved.

But apart from these two minor problems, I must conclude that the Garrison chapter makes an important contribution to Austrian theory.

Hutchison

In "Philosophical Issues That Divide Liberals: Omniscience or Omni-nescience about the Future?" Terence Hutchison provides an insightful, detailed, and useful survey of the economic literature regarding assumptions of knowledge. He covers such schools of thought as the physiocrats, the French Enlightenment, the utopians, the classical economists, the neoclassicals, the Rational Expectationists, the institutionalists, the Austrians, and the neo-Austrians. In this brief (seventeen-page) essay, mention is also made of the following economists: Popper, Mises, Smith, Mandeville, Tucker, Galiani, Viner, Quesnay, Malthus, James Mill, Ricardo, McCulloch, J.S. Mill, Senior, Walras, Cairnes, Wieser, Robbins, Leslie, Nasse, Menger, Knight, Keynes, Hayek, Shackle, Lachmann, and Coase.

Not only is Professor Hutchison exhaustive, he is also largely correct in his analysis. Briefly, his thesis is that most economists and schools of thought have highly overestimated the knowledge possessed by the economic actor and have been altogether too ambitious in their claims in behalf of the economics profession. These "optimistic assumptions, or pretensions" were not made so much by Smith, Mandeville, Tucker, and Galiani, as they were by Quesnay, the physiocrats, both Mills, Ricardo, and McCulloch. The "perfectly competitive economy," beloved of the neoclassical school, and Walras, along with the Rational Expectationists, "mathematical and quantitative methods," and the assumption of full or perfect knowledge all come in for some particularly heavy criticism in terms of claiming more than is justified or being particularly unrealistic.

Our author is especially and quite properly critical of the notion that "even if ignorance might be conceded as an important characteristic of individuals, knowledgeable or even omniscient governments could correct or compensate for their errors and maintain an equilibrium" (p. 128).

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If Hutchison is derogatory about the overestimation of knowledge in economics, this is also true for his views regarding its underestimation. In this context he castigates the neo-Austrian views of Shackle and Lachmann: "They seem . . . to depend on assumptions, or arguments, which constitute too extreme a reaction from conventional, utopian superationalism towards a kind of subrationalism, or even irrationalism" (p. 130). This, I submit, hits the nail exactly on the head.

The essay also features a fascinating discussion of the relationship between knowledge (or ignorance) and the possibility of human freedom. Hutchison concludes, quite wisely, that "significant freedom can hardly exist at the poles of omniscience and omni-nescience about the future, but can only emerge in a mixed world . . . which . . . combines elements of predictability and unpredictability" (p. 131).

By now, the reader will have deduced that I liked this contribution quite a bit. However true, there are still a few nits to pick with its author. First of all, Hutchison carries his war against certainty to an extreme on one occasion by denying that there can ever be incontrovertible economic knowledge. Siding with Hayek vis-à-vis Mises, Hutchison states: "In other words, apriorism cannot tell us anything about real world causation" (p. 128). But consider the statement: "On the free market, there is a tendency for profits to equalize in different industries, given that account is taken of risk." This (or any correct tendency claim, for that matter) would appear to be certain, at least in the sense that no state of the world could falsify it; as well, not only does it tell us something about real-world causation, it explains an essential element of real-world economic causation.

Second, an equation is made between "a libertarian school of economic thought" and the neo-Austrianism of Shackle and Lachmann (p. 129). On an aesthetic level, perhaps a better synonym would have been *libertine* rather than *libertarian*. In a more serious vein, libertarianism is a political philosophy, while Austrianism is an economic school of thought. If the normative-positive distinction is to be maintained, as it should (my value judgment), equations of this sort are problematic.

Third, there is a difficulty with Professor Hutchison's view that it is question begging "simply to deny the 'possibility' of the predictions on which some kinds of governmental policies might be based, while claiming the possibility of the kind of predictive capacity required . . . for the effective operation of smoothly self-adjusting markets." But Austrian economists do not "simply . . . assert that one kind is, in principle, impossible, while the other kind is intellectually respectable and well worth attempting" (p. 132). On the contrary, they prove this with a wealth of analysis, showing how the different institutions, government and the market, operate in very different ways. (See Rothbard, 1977.)

But these points occupy a minuscule part of the essay, which is well worth reading, and makes an important contribution to our store of knowledge.

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Kirzner

This is yet another brilliant contribution to the Austrian edifice, one to which Israel Kirzner has already given so much. In this paper on subjective costs, our author draws heavily on the insightful work of the Austrian James Buchanan[14]—the better to criticize that of the neoclassicals Armen Alchian, George Stigler, and William Baumol.

Like a magician pulling rabbits out of a hat, Kirzner begins by distinguishing no less than four different interpretations of cost. Consider the case of a swimming pool. First, there are the disadvantages of building it, such as the reduction of one's bank balance and the attendant noise created by unruly neighborhood children. Second is the objective opportunity forgone, such as a car, which can no longer be acquired. Third is the objective alternative that could have been produced with the raw materials that instead went into the construction of the pool (for example, a summer cottage). Fourth, and most important, is the subjective "assessment at the moment of his decision regarding the pool of what he would be giving up" in order to obtain it, not in terms of a definite item, such as an auto or a cottage, but in terms of the "utility prospects deliberately sacrificed" (pp. 142–43).

In Alchian's view, the opportunity cost of the pool is either the objectively known car or the cottage; it is definitely not the essentially subjective loss of utility imposed by the noisy neighborhood children. However, in Kirzner's subjectivist approach, this by no means follows. On the contrary, it is possible that the noise is one of the perceived prospects that are knowingly and willingly given up at the moment of choice. It all depends upon the mindset of the *chooser*; it is not and cannot be given to outside observers such as Alchian.

In order to clarify this distinction further, Kirzner next takes up the case of the "expensive wife." In the view of George Stigler and others (p. 145), the cost of marrying a woman (who stays at home to raise children) is the opportunity cost of the salary she forgoes by not working. Therefore, it is cheaper to marry an uneducated female, with poor employment prospects, than her highly market-skilled sister. States Kirzner in reply:

> Before this marriage decision the prospective groom had no alternative prospect whatever of enjoying the woman's high professional income; his decision to marry her involved *no sacrifice* by him of her income at all. . . . To be sure, once the two have married, a *subsequent* decision that she stay at home carries with it the cost of her foregone [*SIC*] income. But this is irrelevant to the injunction to the would-be groom to marry the uneducated girl in order to avoid high costs. (p. 145)

Continuing his critique of the notion that the outside observer can determine costs, our author considers two homeowners in similar circumstances

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who are contemplating constructing identical swimming pools. He gives four reasons for supposing that they may have *different* costs (1) One homeowner may forget to incorporate the noisy-kid factor into his calculations. (2) They form different expectations as to the likelihood of this inconvenience. (3) They may attach different weights to its importance. (4) The outside observer faces not just practical impediments in his determination. "To rank the costs faced by different decision makers is as conceptually impossible a task as is that of comparing utilities interpersonally. (In fact, of course, these two tasks are merely variants of a single impossible undertaking.)" (p. 147)

Next, it is Baumol's turn under the gun. In his view, money outlays can measure subjective costs. If it costs twice as much to kill a beaver as a deer, then one beaver should naturally exchange for two deer. Thus, the subjective costs of one beaver, for everyone in a flexible marketplace in equilibrium, would be two deer, no more and no less. This line of reasoning is rejected by Kirzner on the grounds that it is not enough to talk about the sacrifice of "*objects*, as opposed to *perceptions* of these objects" (p. 150). In the two-commodity world of deer and beaver, it is perhaps tempting to do just that. But in the real complex world, we cannot assume that two people will look upon objects in the exact same way, even if the objects are physically identical. "If the cost notion is to serve as an *explanation* of why a person made the decision he did, it will not do . . . [to make this equation] . . . unless we can rely upon the assured, complete awareness of the objective facts" (pp. 150–51). Baumol, in other words, is assuming away the possibility of error in the learning process, and it is only through this process that we have any hope of achieving full awareness.

Our author concludes his analysis with a consideration of the doctrine of social costs. Since cost has no meaning outside of and apart from an actual decision, and the act of choice can only be an individual one, all subjective costs must of necessity be private ones. To the extent that the concept of social cost has any meaning at all, it must refer to hypothetical (and thus metaphorical) decisions, not real ones. And this goes for the so-called objective opportunity costs as well. The core of any meaning they may have can only be made intelligible in terms of subjectivity.

Although Kirzner does not mention the political implications of his work, they are not too difficult to discern. Since outside observers are never in a position to determine the costs of actual market participants, all public policy that implicitly relies on objective costs (e.g., tax, antitrust, and utility legislation) is without intellectual foundation.

No analysis of any article would be fully complete without a mention of its shortcomings, and in this case the faults were obvious and dramatic. First of all, I felt compelled to read through this chapter on four separate occasions (and, of course, profited immensely from each perusal). Second, I felt compelled to underline virtually every single sentence—many several times over,

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in different colors of ink—and to write congratulatory notations in the margins throughout. After a while, my copy of this chapter was unreadable and I had to obtain another book. If you wish to see a brilliant mind in action, to gain a never-to-be-forgotten insight into praxeological reasoning at its best, and have alternative costs of time that preclude extensive study, then read this chapter. It is truly exquisite.

Mittermaier

Anthropomorphism is the assignment of human mental processes to a non-human entity. When this is done in behalf of objects, such as stones, trees, storms and molecules, it is illegitimate. In more superstitious times, this was common practice. Nowadays, of course, the physical sciences have exposed anthropomorphism as a fallacy and have banished it. However, the rejection of this doctrine in that area has become so well entrenched, and those who have done so have taken on such a great aura of prestige that as a result, a horde of imitators has arisen in the social sciences. Because of their efforts, this precept has been not only expunged from the physical sciences, where it does not belong, but from the human sciences as well, where it does.

Karl Mittermaier has designated such later development as a "mechanomorphism" and defined it as improperly "ascrib[ing] mechanical properties to what is otherwise recognized as an aspect of human affairs" (p. 237).

In criticizing the mechanomorphisms of the positivist economists, Professor Mittermaier makes an important contribution to Austrianism. He is particularly sharp and critical with regard to macroeconomic aggregates, which he characterizes as "treat[ing] an economic system as though it were a mechanical system" (p. 237). He captures precisely the orthodox view on saving and consumption as "a macro-economic fluid whose flow sometimes has to be augmented and sometimes diminished" (p. 238).

In his analysis, to slavishly and uncritically copy classical mechanics from the physical to the social sciences is to commit the logical fallacy of violating the coherence rule (p. 247). This rule "stipulates that a question should be posed in terms which all belong to the same domain of thought and that the corresponding answer should be composed in terms which all belong to the same domain of thought as the terms of the question" (p. 247).[15] That is, I take it, if we are discussing actual people with beliefs, fears, and expectations, people who make purposeful decisions, then by all means do not conflate this category of discourse with talk of indifferent curves, "utils," and other such tools of the trade of the modern mechanomorphic neoclassical economist.

Important as are these positive elements of his paper, it is marred by a tendency to go to the other extreme and dismiss as mechanomorphism several concepts that can be strongly grounded in human purposiveness. Consider

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"equilibrium," for example. The author of this chapter asserts that "Equilibrium clearly comes from this domain of thought, and talk of equilibrating or market forces must be regarded as mechanomorphic" (p. 237). There is no doubt that in the hands of *some* economists, equilibration is treated as no more than a balancing act, akin to that attained by a weight scale. But as others use the concept, equilibrium (or the ERE) is interpreted in terms of purposiveness or human action.

The concept of time would appear to be another instance. Mittermair objects to its use in economics because it may be "easily interpreted as a prediction" (p. 238). Yes, to be sure, there is always the danger that time may be used mechanomorphistically, completely apart from human motivations, but our author comes perilously close to asserting that this *need* be the case.

Another difficulty with the chapter is that our author complains that Mises devoted only "about 300 words" (p. 246) to the explication of the concept of human action. He realizes that the elaboration can be found in *Human Action*, but is unhappy with the fact that one would need a "fine-tooth comb" (p. 247) to find it. He seems unaware of the contribution of Murray N. Rothbard (1962) in this regard.

Further, Mittermaier holds that private property implies means and ends, but denies the obverse (p. 247). Yet, how is it possible to have means and ends, surely the result of human purpose, without private property?

White

"A Subjectivist Perspective on the Definition and Identification of Money" by Lawrence H. White is a gem of an article. It carefully sets up praxeological considerations and utilizes them to address a perplexing problem that would otherwise be far more intractable.

In the first section, the University of Georgia professor grounds his analysis in the subjectivist perspective; he will seek a monetary definition comprehensible in terms of individual human action and purpose, not based on correlations with any statistical aggregates. He arrives at GAMOE, the "generally accepted media of exchange" in an economy. White shows how the other textbook definitions of money (such as, store of value, unit of account, and standard of deferred payments) are all subsidiary to money's essential nature of facilitating exchange.

In terms of actual identification of money candidates, he concludes in section 2 that only hand currency, coins, traveler's checks, and checkable claims on banks pass muster; that is, he arrives at that old warhorse, M1. He specifically rejects claims made in behalf of the moneyness of noncheckable bank liabilities and money-market mutual fund shares.

White is particularly eloquent and forceful in rejecting Yeager's objections to the inclusion of traveler's checks in the money supply. He does so on the

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grounds that the criterion of repeated circulation (which this medium lacks) would rule out of court, under a gold standard, those parts of gold bullion intended to be used not for exchange, but for industrial purposes. As well, the narrower Yeager criteria of routine circulation would reject corn as money, even though it were the most popular exchange medium in a barter system, since some of it could be eaten. This would also repudiate "checkable demand deposits in a multibank system . . . [since] demand deposit claims on Bank A are not accepted by customers of other banks with the intention of passing them along without redemption" (p. 307).

Unfortunately, however, White does not spend nearly as much time criticizing the views of Rothbard, a more serious protagonist. Instead, in the space of an exceedingly short footnote, White cursorily dismisses the Rothbardian claim that passbook or savings accounts should be considered money on the ground that they can be turned in for cash on demand. The author of this chapter maintains that "this feature is irrelevant when they fail to satisfy the medium of exchange criterion for money which Rothbard (1978, p. 144) himself enunciates" (p. 314, note 26). One question that remains unresolved, based on this reply, is whether or not passbook accounts redeemable on demand constitute money under the medium of exchange criterion. It would appear that such funds could be used to transact business and that at least some people actually do so. Another inquiry that remains open, unhappily, is how White would respond to what can only be considered Rothbard's attempt to anticipate this very objection:

> Suppose that, through some cultural quirk, everyone in the country decided not to use five-dollar bills in actual exchange. They would only use ten-dollar and one-dollar bills, and keep their longer-term cash balances in five-dollar bills. . . . If a man wanted to spend some of his cash balance, he could not spend a five-dollar bill directly; instead, he would go to a bank and exchange it for five one-dollar bills for use in trade. In this hypothetical situation, the status of the five-dollar bill would be the same as that of the savings deposit today. But while the holder of the five-dollar bill would have to go to a bank and exchange it for dollar bills before spending it, surely no one would say that his five-dollar bills were not part of his cash balance or of the money supply. (Rothbard, 1976, p. 181)

While this paper otherwise makes a sterling contribution to Austrian thought and to monetary theory in general, there are, in addition, two very minor difficulties. First, on pp. 301–2, its author positions himself strongly in the subjectivist camp, criticizing econometric analysis and the statistical and aggregative perspectives as not in keeping with the individuals' plans. But on page 304 we find him announcing that his work will be of "vital importance" for statistical work, for price indexes, and for other aggregates. And so it will be. Paradoxically, even the use of macroeconomic concepts can benefit from

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methodologically individualistic insights. But one must consider it a lacunae that no discussion of this seeming contrariety is provided, nor is there any critique of this mode of analysis, as would appear consistent with White's own basic outlook.

Second, the author of this paper maintains that his study is "consistent with the methodological subjectivism espoused by Ludwig M. Lachmann" (p. 311). This, however, is somewhat misleading. On the one hand, there is nothing in this chapter that is uniquely connected with Professor Lachmann's version of subjectivism. Any other Austrian version of subjectivism, individual purposiveness, or human action would suffice at least as well. On the other, a case could easily be made out that the Lachmann "kaleidic" vision is *not* compatible with White's work. For suppose that expectations are not convergent. That is, assume with the honoree of this Festschrift that people diverge in their expectations about everything under the sun. How, then, would it be possible to construct a comprehensible theory of money, as White has so admirably done, given that implicit in its identification is not merely individual subjectivism, but rather intersubjectivity? If, moreover, inherent in the definition of money is an intention to use it as a medium of exchange in the future, but no one can ever know anyone else's future intentions or expectations, how can we even coherently define money, much less identify any?

Conclusion

Due to my focusing the detailed analysis on by far the best chapters in this Festschrift, the reader may be left with a better impression of the book than it deserves. In point of fact, this volume is replete with errors and is most disappointing for a book touted as the most modern, up-to-date version of Austrianism and as a challenge to the economics of Mises. Indeed, the majority of its chapters simply ignore the basic tenets of this school of thought. And several of those that do not ignore praxeology misconstrue it.

But it is important that an overall assessment of the work of the followers of Professor Lachmann not be lost sight of amid the welter of minutiae about their specific errors. And, unfortunately it is not just that they are wrong about equilibrium, methodology, hermeneutics, time preference, and so on; even worse, if possible, is the fact that there has been a virtual cessation of focus on real economic problems such as money and banking, business cycles, utility and welfare economics, and monopoly theory. Virtually all that is heard from this quarter is an endless repetition that the market is a process, that equilibrium is a red herring or worse, that no one can ever know anything, and that all is subject to interpretation.

It is possible to construct a continuum in this regard. On the one extreme would be the mainstream empiricists, who believe that their regression equations

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can test economic axioms. This might well be called an overreliance on economic research. On the other extreme would be the followers of Lachmann, who have all but eschewed economic research; the impossibility of knowing the future, the divergence of expectations, radical subjectivism, it would appear, make this an extremely dubious path for them to follow. And as moderates on the continuum are the Misesians, who maintain that history can illustrate but not test theory, and who are nevertheless vitally interested in doing just this type of work.

Notes

1. All page references, unless otherwise identified, are to this Lachmann Festschrift.

2. It is a personal pleasure to be able to review the Lachmann Festschrift and thereby be able to honor this great man, however indirectly, because as it happens, I, along with Walter Grinder, was able to play a small role in support of the initial invitation that first brought him to NYU as a visiting professor.

3. S.C. Littlechild, *Economic Affairs*, June 1987, p. 43. Similarly, David L. Prychitko, "Ludwig Lachmann and the Farther Reaches of Austrian Economics," *Critical Review*, vol. I, no. 3, p. 66, makes the point that this book is "in the Austrian tradition" and claims that "For anyone interested in the farther reaches of Austrian economics, this is a book worth reading."

4. The magnificent contribution of Garrison on this subject serves as an antidote to these musings; unfortunately it is confined only to his chapter.

5. To be sure, this Baird criticism was published one year after the publication of the Rizzo-O'Driscoll chapter in the Lachmann Festschrift, in 1986. But an earlier draft of the Baird paper was presented to O'Driscoll and Rizzo at a Liberty Fund Conference on Time and Ignorance in November 1984.

6. In his fourth footnote, High claims that nothing in George Selgin's "Praxeology and Understanding" (unpublished manuscript later published as Selgin [1987]) "contradicts the defense of Lachmann presented here." But this is hard to reconcile with the criticism that Selgin actually leveled at Lachmann (see especially pp. 32–33). Professor High also claims in this footnote that "Selgin's views are much more compatible with those of Lachmann than his paper would suggest." In this, he is contradicted by his colleague at George Mason University, Don Lavoie, who correctly asserts in his first footnote: "George Selgin's critique of Lachmann and Shackle, which interprets them both as historicists, points out that since the Austrian school began with Menger's devastating criticism of the historicists of his day, it would be unfortunate if contemporary Austrians were to revert to this antitheoretical stance." While Lavoie mistakenly disagrees with Selgin's contention that Lachmann is an example of historicism, he at least sees, as High does not, that in *Selgin's* view this is so.

7. Notice how truly great a departure is this view from those we have come to associate with Professor Lachmann. Usually, and typically, we may rely on that eminent economist to express a skeptical view, not to say a nihilistic one. But in this case, in stark contrast, he is amazingly exuberant and optimistic about the knowledge and abilities attained by our profession.

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8. This is a tautology, not an empirical claim. For if we did not know anything about x, we would have to be completely unaware of it. And if that were true, the question of our knowledge about it could not even arise. And if we were even but aware of x, we would know something about it, namely its existence.

9. See Selgin (1987, pp. 34–36).

10. For Lavoie, "formalism" is equated with the neoclassical fixation on equilibrium states, which he now dubs "Euclideanism" (p. 193). In contrast, there is "antiformalism," which is now equated with hermeneutics. Thus, with a sleight-of-hand redefinition, there would appear to be no room for a Mises who rejects both neoclassical formalism and hermeneutical antiformalism. One must, by definition, be either a neoclassical Euclidean formalist or an "Austrian" hermeneutical antiformalist. To paraphrase the old adage, "When faced with needless and extraneous definitions, reach for your wallet—or count your fingers." There is a complication, however. Our author also characterizes Euclideanism as a "fully axiomatized, linearly constructed system of strict deduction" (p. 198), which is a far more apt description of Mises.

11. According to the survey conducted by Frey et al. (1984), 10 percent of U.S. economists disagreed with the proposition that "A minimum wage increases unemployment among young and unskilled workers," and 1.9 percent denied that "A ceiling on rents reduces the quantity and quality of housing available."

12. For example, Lavoie (p. 207) cites Mises as follows: "In asserting the a priori character of praxeology we are not drafting a plan for a future new science different from the traditional sciences of human action." Lavoie concludes from this that Mises could not have meant to refer to "strictly linear deduction" (p. 207). But in the very next sentence following the one quoted, Mises maintains not "that the theoretical science of human action should be aprioristic, but that it is and always has been so" (*Human Action*, p. 40). Lavoie seeks to support his interpretation of Mises as a sort of empiricist, not a praxeologist, when he quotes him as stating: "All that man can do is to submit all his theories again and again to the most critical re-examination" (p. 207). Very reasonable. But again Mises' very next sentence supplies the crucial context neglected by Lavoie: "This means for the economist to trace back all theorems to their *unquestionable and certain* ultimate basis, the category of human action . . ." (*Human Action*, p. 68, emphasis added). Contrary to Lavoie, Mises is not looking for greater certainty about the world from more and more empirical induction; rather, he is urging us to rigorously check our basic premises and the logic of our deductions. Further, Lavoie disingenuously interprets Mises' statement "Economics does not follow the procedure of logic and mathematics" (p. 205) as support for his contention that Mises did not mean "for economic theory to be forced into a fixed, hierarchically deductive structure, like the theorems of geometry" (p. 205). But anyone who reads the relevant passage from *Human Action* (p. 66) will discover that this is not at all what Mises stated. Yes, to be sure, there are important differences between geometry and praxeology. But this has nothing whatever to do with hierarchy and deduction, as misleadingly adduced by Lavoie. On the contrary, the differences have to do with the assumptions, the applications, and the "solutions of concrete historical and political problems" (*Human Action*, p. 66). Geometry, in other words, is concerned with spaces and lines; praxeology, with acting human beings. But apart from that, their methodologies are very similar.

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13. I have interpreted Lavoie as claiming Mises as a hermeneutic anticipator. According to an alternative perspective, Lavoie argues, in effect, that Ludwig von Mises had a bipolar personality disorder, a species of philosophical schizophrenia. As a result, Mises could not decide between formalistic certainty and radical skepticism. In this view, when Mises speaks of "apodictic certainty," Lavoie puts Mises on the couch and sees only self-congratulatory flourish. When Mises addresses the need for careful thought, psychiatrist Lavoie diagnoses this as questioning the fundamentals of economic law. This interpretation is buttressed by Lavoie's habit of dismissing Mises for using empirically based arguments when he is merely utilizing the methodology of his opponents in order to employ *reductios ad absurdum*. For example, Lavoie (p. 206) interprets Mises as a pragmatist ("we accept this pragmatic point of view") but, as a perusal of the actual page of *Human Action* (p. 24) makes clear, Mises was only employing this view as part of a *reductio ad absurdum*. Mises adopted pragmatism, but only temporarily, for the sake of argument, in order to overturn this philosophy.

14. James Buchanan may not be an Austrian on most other questions, but he certainly is with regard to subjective costs. See his *Cost and Choice* (1969).

15. His critique of the failure of the mainstream orthodoxy to be bound by the coherence rule is dramatic and incisive. He states:

> The student is introduced to a topic reeking with the richness of social life. He is then taken by a little legerdemain through a blur and suddenly finds himself in an eerie world of continuous functions. He watches the functions shift about and, when they have stopped, notes down the coordinates of their points of intersection. He is then taken again through the blur and, behold, he finds himself once more among familiar human faces. The recommendation of this paper is that the subjectivist case against mechanomorphism be based on the ideal that such blurs be removed. (p. 249)

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Book Reviews

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You'll Find It in *The New Palgrave*

Mark Skousen

Free-market economists, especially of the Austrian variety, have faced an uphill battle in gaining recognition in the post-Keynesian world. For many years it was difficult, if not impossible, to be published in the prestigious journals, to teach at major universities, to participate in academic conferences, to contribute to edited books, or just to be mentioned in introductory college textbooks. In short, the economics profession was largely antagonistic toward the tiny band of laissez-faire advocates.

Fortunately, this trend is gradually reversing itself. Hayek became a Nobel laureate in 1974, and a number of "think tanks" (among them the Cato Institute, the Adam Smith Institute, and the Mises Institute) began publishing free-market Austrian views. Murray Rothbard, who for many years taught at the little-known Polytechnic Institute of Brooklyn, is now a professor at the University of Nevada at Las Vegas. The Mises Institute is established at Auburn University, where Leland Yeager and Roger Garrison offer a graduate program in Austrian economics. Other universities that have been receptive include George Mason University, New York University, and the University of Dallas. Two academic journals, *History of Political Economy* and *Southern Economic Journal*, have been fertile ground for Austrian submissions. And even some of the new introductory college texts are including biographies of Mises and Hayek. See, for example, Ralph T. Byrns and Gerald W. Stone, *Economics*, 3rd ed. (Glenview, Il.: Scott, Foresman, 1987). Sheila C. Dow, in her pathbreaking work, *Macroeconomic Thought: A Methodological Approach* (Oxford, England: Basil Blackwell, 1985), includes the "neo-Austrian" view on an equal basis with other schools of thought (post-Keynesian, Marxian, and mainstream). I hope her approach will be imitated elsewhere.

Unfortunately, the battle for complete recognition is not over. There is still a great deal of prejudice against the followers of Mises and Hayek, especially in terms of obtaining academic positions and publishing in the most important

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Review of *The New Palgrave: A Dictionary of Economics*, John Eatwell, Murray Milgate, and Peter Newman, eds. (London: Macmillan; New York: Stockton; Tokyo: Maruzen, 1987).

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economic journals. Murray Rothbard is not even listed in Mark Blaug's otherwise exhaustive *Who's Who in Economics* (Cambridge, Mass.: MIT Press, 1986). Blaug's explanation for this grave oversight is that Rothbard was not cited sufficiently in the academic journals to be included in the list of one thousand famous economists. Radical economists have a difficult time fitting traditional standards.

Another example: a recent scholarly work, *The Gold Standard in Theory and History*, edited by Barry Eichengreen, which Peter Temin at MIT called "the best collection of readings on the gold standard available today," contains not a single article by a gold-standard advocate, and the index makes no reference to the views of Mises, Hayek, or Rothbard. The editor does make a short reference to *The Case for Gold*, by Ron Paul and Lewis Lehrman (Washington, D.C.: Cato Institute, 1982), but that is all. So much for equal time.

Free-Market Contributions to *The New Palgrave*

Now comes a blockbuster to the economics profession, a monumental reference work that is bound to be widely used and quoted over the next hundred years: *The New Palgrave: A Dictionary of Economics*.

As a financial economist trying to catch up on what has happened in academic economics over the past few decades, I found *The New Palgrave* to be both comprehensive and fascinating. Blaug's *Who's Who in Economics* pales by comparison. *The New Palgrave* is an updated version of the century-old *Dictionary of Political Economy* (London: Macmillan, 1894, 1896, 1899), edited by Inglis Palgrave, an English banker and editor of *The Economist* in the 1880s.

But this is much more than an update. *The New Palgrave* is 4 large volumes, over 4 million words, and 3,500 pages long. It contains 700 biographies and 2,000 entries. Over 900 economists contributed to the reference work. As would be expected, the price for the set is also enormous: $650!

There is no question in my mind that *The New Palgrave* will be a success as a single reference work to economics and an indispensable tool for researchers and students. Imagine, turning to a single resource and finding original articles by Milton Friedman, Paul Samuelson, James Buchanan, Kenneth Boulding, George Stigler, James Tobin, John Kenneth Galbraith, and Murray N. Rothbard.

Followers of the free market should not be disappointed in this exhaustive dictionary. There are numerous references to the views of the Austrian, Chicago, or new classical (Rational Expectations) tradition. Unlike most other dictionaries and commentaries, which often ignore Austrian economics or write disparagingly about the free market, the editors of *The New Palgrave* let the

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economists speak for themselves. Lengthy contributions are made by such prominent free-market economists as Murray N. Rothbard, Israel M. Kirzner, James Buchanan, Roger Garrison, Karen Vaughn, P.T. Bauer, Thomas Sowell, Milton Friedman, Armen Alchian, and Leland Yeager. For example, Israel Kirzner wrote five and one-half pages on the Austrian school of economics and Murray Rothbard wrote five articles, including two pages on Ludwig von Mises. In all, I counted at least fifty articles written by free-market economists whom I recognized. While this is small in comparison to the other schools that made contributions, it is a promising beginning.

The extensive biographical listings contains a surprising number of Austrian economists, including Carl Menger, Eugen Böhm-Bawerk, Friedrich Wieser, Ludwig von Mises, Freidrich Hayek, Fritz Machlup, Frank A. Fetter, Wilhelm Röpke, Jacques Rueff, and even Richard Strigl. (I was disappointed, however, that Ludwig M. Lachmann and W.H. Hutt were omitted; both qualified in terms of being born before 1915.) The biographical details are far more expansive than those found in other reference works.

Keynesian Bias in *The New Palgrave*

I am not saying that *The New Palgrave* is without serious faults. Overall the material is heavily weighted against a free-market approach. *The New Palgrave* reflects the fact that Keynesianism and all its many forms still dominate the economics profession. Admittedly, there are significant sections on monetarism, the new classical macroeconomics, and Marxism, but the major thrust of the dictionary is definitely Keynesian. Two of the three editors (John Eatwell of Cambridge and Murray Milgate of Harvard) are Keynesians, while Peter Newman of Johns Hopkins is an econometrician. This perspective is reflected in the fact that the biography of John Maynard Keynes is twenty-three pages long, followed by three subsequent entries: "Keynes's General Theory," "Keynesian Revolution," and "Keynesianism." None are written from the viewpoint of an opponent to Keynesian economics.

There are numerous examples of editorial bias. The entry on "capitalism" is penned by Robert L. Heilbroner, a socialist who refers more to Karl Marx than to Adam Smith in the essay. Heilbroner's bibliography includes numerous references to Marxists and socialists (including Paul Baran, David Gordon, and Gunnar Myrdal) and only one reference to a defender of the market (Adam Smith). An essay on the subject of "price control" is written by John Kenneth Galbraith, a proponent of wage-price controls. The entry on the "gold standard," like Eichengreen's book referred to previously, does not include any pro–gold-standard works in the bibliography.

Of course I have no objection to the editors' inclusion of Heilbroner and Gailbraith's essays; they are quite provocative. But should they not have asked

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Hayek, Friedman, or Rothbard to write a contrasting article? The dictionary contains three articles on competition (Austrian, classical, and Marxian) and four articles on distribution theories (classical, Keynesian, Marxian, and neoclassical). Why not offer opposing views on capitalism, price controls, the gold standard, communism, depression, inflation, development economics, and other controversial areas? Such a comparative approach works well in Sheila C. Dow's *Macroeconomic Thought*. The editors of *The New Palgrave* have made an attempt at fair play by having fee-market economists write on some of their favorite topics (e.g., Karen Vaughn on the "invisible hand," Murray Rothbard on "time preference," and Steve Hanke on privatization) but this even-handed approach is not consistently followed.

Still, the benefits of this reference bible far outweigh the disadvantages, even for economists holding a minority position. The vast list of biographies makes it an indispensable research tool. One of the unusual sketches I found interesting was on Augustus M. Kelley, the publisher of many free-market books that have gone out of print (such as Hayek's *Prices and Production*); another was on Robinson Crusoe, the fictional character created by Daniel Defoe and used so frequently in earlier economics textbooks by Menger, Böhm-Bawerk, and Bastiat, among others, to explain the fundamentals of production, labor, time preference, and so on.

Moreover, for anyone who wants to get a rudimentary understanding of the various conflicting theories in economics today, there may be no better source than *The New Palgrave*. In many cases, the topic is addressed by a prominent proponent of the theory. As the editors state, the dictionary's objective is "not to produce a synthesis of current thought, but to provide an opportunity for leading authorities in economics to present accurately and forcefully their own analysis and arguments." Examples include the "quantity theory of money" by Milton Friedman and "input-output analysis" by Wassily Leontief. (But, strangely enough, new classical macroeconomics is written by a critic, MIT's Stanley Fischer. A better choice would have been Robert Lucas.)

Each entry is supposed to be written in a scholarly, comprehensible, and clear fashion. The numerous mathematical expositions are demanding, even on an elementary level, and a difficult challenge in clarity, to say the least. *The New Palgrave* reflects the overwhelming influence of econometrics and mathematics in the economics profession. The journals already assume a basic understanding of the mathematical models, a serious drawback for the student who wants to know the point of the article. Now he can turn to *The New Palgrave* for a short course in the subject. For example, if a journal article refers to "Bayesian inference," you can look it up in the dictionary. This does not necessarily mean that you can fully understand the topic, but it provides a beginning.

Finally, the reason I like *The New Palgrave* so much is that it is an expression of modern economic thinking on almost all subjects. It is a critical

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introduction to the wide world of economics that you could not get anywhere else without spending a great deal of time and effort in the library jungle. Here in one location you can learn about new books, articles, topics, people, and theories that you might never discover by pouring through the stacks of libraries throughout the country. And for that we can thank the editors and publishers of *The New Palgrave.*

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The Origins of Language: A Review

David Gordon

Before considering *The Origins of Language,*[1] a preliminary question confronts us. Wells' book is a historical and critical account of eighteenth- and nineteenth-century theories of the origins of language: "aspects of the discussion from Condillac to Wundt," in the words of its subtitle. Why then discuss this volume in *The Review of Austrian Economics*?

The answer to this query lies in the value of Wells' investigation in placing in question an idea many Austrians, under the influence of F.A. Hayek, have blown up out of its real but subordinate place in social explanation. Hayek has made famous a phrase of Adam Ferguson, "the results of human action but not of human design," and used the notion this slogan encapsulates as a principal tool of analysis of economic and social institutions.[2]

In the first volume of Hayek's *Law, Legislation and Liberty,* for example, he denies that the systematic character of law comes about because people designed the legal system in the style of Euclid elaborating his geometric theorems from his definitions and axioms.[3] Quite the contrary, law arose from decisions by particular courts. Drawing most of his examples from British law, Hayek maintains that as courts modified and adapted the decisions of preceding judicial bodies, an organized assemblage of law developed in the course of centuries. No one planned the legal system. Like Topsy in *Uncle Tom's Cabin,* it "just growed."

Of greatest significance to economists, of course, Hayek uses the notion of unplanned order to explain the market economy. In contrast with centrally directed socialism, the market permits people to coordinate information that no one person possesses. Moreover, not only does the market bring information together in an undesigned yet at the same type systematic fashion; the key to its genesis lies in the same direction. No one planned the rise of market capitalism. Like the common law, it too arose "as freedom slowly broaden[ed] down . . . from precedent to precedent."

Although Hayek presents what I have just summarized as a package, the preceding paragraph in fact contains two distinct theories. For the first of these,

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Review of G.A. Wells, *The Origins of Language* (Peru, Ill.: Open Court, 1987).

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everyone sympathetic to Austrian economics must be grateful to Hayek. Following his teacher, Mises, he showed that only in a market system can efficient economic behavior take place. Central "planning" leads to chaos.[4] But it does not follow from the fact that central planning cannot work that people who use the market mechanism operate in a fog of ignorance. They may consciously desire to have a market system, and their coordinated action in maintaining it is then *not* an "unintended consequence of human action." They may avoid harmful intervention, not because they blindly follow traditional rules, but because they understand the way the market works.

We may extend this point one further step. Not only can the market *now* operate as the result of the deliberate plans of people, it may for all Hayek has shown have originated by conscious planning. Nothing in Hayek's analysis of the way the market coordinates information forbids people to establish a market system through agreement.

I do not contend that Hayek errs historically in seeing the origin of either common law or capitalism as the outcome of an evolutionary process designed by no one. To evaluate his thesis would be a long and complex undertaking—one would have to assess, for example, Walter Ullmann's contention that much of medieval law was centralized—and in any case this is not to the point here.[5] Rather, I wish only to insist that Hayek's analysis of the market does not entail that capitalism, or by analogy anything else, arose in the way Sir John Seely said the British Empire was acquired: "in a fit of absence of mind."

Here precisely lies the value of Wells' book for economists. Hayek instances language as a chief example of a social institution that displays a complex order yet has not been invented. Wells stands squarely against this contention. He thinks that language *is* an invention. If he is right, Hayek's view of the undesigned nature of institutions errs in one major instance.

But in turning now to a description of Wells' challenging thesis, I do not intend anything as ambitious as a refutation of Hayek's entire approach to social evolution. Rather, by presenting Wells' contrasting contention, I aim only to bring to the attention of those under Hayek's influence that alternatives to his analysis do exist. Even if Hayek's stress on nonconscious evolution is correct, his position needs to be defended by argument rather than asserted as self-evident.

But how can language be a deliberate invention? As Wells points out (p. 11), the fundamental obstacle that confronts any theory of the invention of language is a paradox succinctly stated by Jean-Jacques Rousseau: "Words seem to have been necessary in order to establish the use of words." To invent a new word is an easy task, once people already speak a language; but how can an entire language be invented? Invention implies conscious thought, but how can one construct a language, when thinking presupposes the use of words? Perhaps Hayek is right after all.

Wells ably brings out the way in which several writers during the Enlightenment—principally Condillac, Reid, and Monboddo—challenged the

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seemingly watertight argument just offered. The trouble comes in the last premise of the paradox: *does* thinking presuppose the use of words? If it does, language cannot be invented. But why *must* thinking be verbal?

As Condillac in particular noted, people can communicate by gestures as well as words. He contended that language *can* then be invented if a stage of communication by gestures preceded the use of words. A member of a group without a verbal language might, for example, wave his arms should he see immediate danger ahead. If successful in his communication, he might avert serious danger to the entire group. Members of the group might then gradually build up an entire repertoire of gestures for various situations, sufficient to constitute a language. From this base, the invention of words seems much less paradoxical. Creation of language thus does not presuppose the prior existence of words.

But does this "solution" do anything more than press back the problem by one step? If people can use gestures to communicate, how did gestures acquire their meaning? Does not the claim that people can understand gestures without words assume just the command of meaning it is supposed to explain?

Here exactly lies Condillac's most original contribution, a point that Wells maintains previous scholars have failed to stress. Condillac by no means claimed that every gesture must be invented. Some are "natural signs" in the way that smoke is a natural sign of fire. To revert to our previous example, to wave one's arms in the face of danger does *not* require a preceding conscious decision. Once people see the effect of natural signs, however, they can go on to use them deliberately. Language, given this indispensable foundation, can avoid Rousseau's paradox.

Once more, though, the question previously raised comes forward in a new guise. Suppose Condillac is right. Grant, even, that there are many gestures that serve as natural signs. It does not follow that people can use these signs in the absence of their immediate stimulus. And surely this is necessary for even a nonverbal language. Unless one can understand a hand-waving gesture (e.g., when no immediate challenge looms), one has no command of language. Even an animal can indicate danger through gestures to other members of its herd. Where then lies the contribution of gestures to the origin of language?

I cannot think that either Condillac or Wells has offered a completely adequate response to this query. But as Wells notes, the situation confronting primitive man differs from that of ants and bees, who communicate in a quite complex way. Human action is not rigidly bound by instinct. Thus, whether or not the use of gestures as a language has been adequately accounted for, at least the space for an explanation stands open. Wells conjectures that the mental capacity required to move to the full use of a language of gestures does not exceed that of the higher apes.

The gap in the theory just decribed can in part be handled by using an approach in another recently published book, J.N. Hattiangadi's *How Is Language Possible?* (coincidentally by the same publisher as Wells').[6] Hattiangadi, unlike

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Wells, does not see gesture as the key to the mystery of language. But, seeking like Wells the origin of language in thoughtful though nonlinguistic behavior, he stresses play as a likely source of the development of language, in a way that fits in with Wells' account. Suppose, to revert once again to our favorite example, those who have averted danger by heeding a hand waver's signal now pretend that danger threatens and "go through the motions" of the episode that they have just survived. This type of play hardly demands much of people and is in fact characteristic of virtually all children. It is exactly the "pretend" element of play that constitutes the importance of this elementary activity for our present purpose. Since, in our imagined case, no immediate danger is present, but the players act as if a threat did confront them, the basis of language learning lies near to hand.[7] The players can represent something that is absent in the same way that through words we refer to objects not on the scene. Hattiangadi's play theory of the origins of abstraction meshes in a remarkable way with Wells' gestural account to offer a most promising theory of the beginning of language.

In urging attention to Wells and Hattiangadi, I do not mean to suggest that all problems in this difficult area stand in sight of solution. One might wonder whether in appealing to the ability to represent absent items, the problem has been solved by assuming it out of existence. How exactly was the ability acquired to engage in play of the imitative sort? Even if an explanatory gap remains, however, on the surface it strikes one as less wide than that involved in the entire creation of a language *de novo*. The origin of language now appears much less intractable.

Before returning to Wells, an objection remains to be considered. This affects not Wells' book but the way I have chosen to present its thesis. If Wells assumes that language evolved from gestures, is he in fact opposing the Hayekian shibboleth, the "results of human action but not of human design"? Wells does not after all contend that people lacking language one day assembled and invented words. Rather, the development of gestures and then words from natural signs takes place in a manner akin to that of Darwinian evolution. What could be more Hayekian?

A crucial difference in fact separates Wells' position from Hayek's model of evolution. When gestures and *a fortiori* words are used to represent absent objects, this process takes place consciously. This use of a sign is nugatory unless its employer realizes what he is doing. Though he may not realize the full complexity of language, he cannot use gestural or verbal language at all unconsciously. If only at an elementary level, he must realize he is using language. Thus, the growth of language differs from the evolutionary pattern, to which Hayek makes constant appeal, in that the latter depends for its success only on everyone following fixed rules. In this pattern, no conscious thought is required. One often gets the impression that for Hayek its presence stands as an obstacle to progress.

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But let us return at last to Wells. After reading his account of Condillac's views and the similar though less developed positions of Thomas Reid and Lord Monboddo, one is likely to be brought up short by a fact difficult to fathom. Although Condillac developed an account of language that seems very much worth extending further, few later writers followed in the path of the Enlightenment writers.

Why not? In part, the answer lies in the lack of anthropological data available until recent times. Bereft of empirical information, the study of the origins of language could hardly rise much above the purely speculative. But this is not the principal cause of the turn away from Condillac's model.

Wells maintains that the main nineteenth-century writers on the origins of language regressed in their understanding from the heights attained in the preceding century. Largely under the influence of J.G. Herder, an important figure of German Romanticism, nineteenth-century writers tended to deny that language could have consciously developed. Ignoring or misunderstanding Condillac, Herder and his successors claimed that Rousseau's paradox—words seem to be required to invent words—was in its own terms irresolvable.

Instead, these writers appealed to the spirit of particular peoples that in some rationally unspecifiable way had generated language. Like Gadamer and our latter-day hermeneuticians, who have in fact been directly influenced by Herder, the appeal to tradition took the place of logical analysis. Hayek's stress on tradition also falls within this framework.

I do not think it necessary to trace the growth of the Romantic theory through each of the rather obscure figures Wells discusses in his valuable historical account. Readers of this journal probably will not be interested in the nuances of the positions held by, for example, Geiger, Steinthal, Müller, and Wundt. (Those who are can consult Wells' book.) But before turning from this group, one more characteristic of their position should be noted.

One reason they rejected the rationalist account was its inability to account for the existence of "higher languages." Largely owing to their grammatical form, some languages could more easily express abstract thought than others. So, at any rate, the great liberal theorist and linguist Wilhelm von Humboldt contended. (Humboldt especially emphasized the use of inflection in the "higher" languages.)

Though pleasing to the ego (since German, not coincidentally the native tongue of most of these writers, usually was ranked as the highest modern language), this notion lacks any scientific basis. Because of the very strong emphasis on it, however, the study of the origins of language received a serious setback from which it has not fully recovered. Those engaged in the quest for the "highest" language were hardly likely to favor a theory that, like Condillac's, viewed language as a relatively easily understood development from certain tendencies universally present among primitive people. The appeal to the essence of a people's spirit and the quest for the Holy Grail of the supreme language went hand-in-hand.

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Rather surprisingly, though J.G. Fichte is twice mentioned (pp. 65, 110), Wells fails adequately to stress the role of this outstanding idealist in the rise of the trend just described. Fichte's *Speeches to the German Nation,* greatly influential as a rallying cry against Napoleon, emphasized the value of German as a so-called *Ursprache,* (a primitive language largely uncorrupted by linguistic mixture), in contrast with the other major European languages.[8]

It is also worthy of note that the quest for the ideal language, far from being universally consigned to the dustheap of lost causes, remains influential today in a certain school of philosophy. I refer of course to Heidegger and his many followers, in short to the camp of hermeneutics. In his *Introduction to Metaphysics,* Heidegger finds the Greek and German languages supreme in their ability to declare the truth of Being.[9] But the mysteries of Heidegger are best left to his hierophants and those aspiring to that dubious status. These murky waters have nothing in common with either the values of the Enlightenment or the Misesian approach to the study of society. I do not think that even so convinced a defender of the wisdom of tradition as Hayek, though his views have in them more than a tinge of Romantic influence, would do other than turn aside from such odd tenets.[10]

I have presented a largely sympathetic picture of Wells' account, and it does seem to me that his approach is more adequate than the currently most favored schools of linguistic origins (e.g., Chomsky's innatism). But one can hardly disguise from readers the fact that Wells' theory is unlikely to receive a warm welcome from the majority of philosophers.

Why not? The answer lies in the fact that Wells' theory flies directly in the face of one of the most influential works on contemporary philosophy, Ludwig Wittgenstein's *Philosophical Investigations.* (Incidentally, Wittgenstein was Hayek's cousin.) According to Wittgenstein, there are no "natural" signs. Any claimed exception (e.g., our case of hand waving in the face of danger) presupposes the existence of a community who have through habit adopted a "language game" in which a sign has a particular meaning. Contrary to Condillac and his twentieth-century successors, language cannot exist in the absence of conventionally accepted rules for the use of terms. Into the labyrinths of Wittgensteinian doctrine I cannot here enter. I raise the point only to support my guess that Wells' theory may unfortunately not have the success it richly deserves. If Wittgensteinian objections do succeed in overthrowing Wells' account, the result will not be encouraging for the study of the origins of language. As Hattiangadi has effectively noted, it is difficult within the Wittgensteinian view to see how language can have originated at all.[11]

Whether or not one is attracted to the gestural account of language origins, however, it is difficult to deny the importance of Wells' book. He has depicted in clear and incisive fashion a nearly forgotten chapter of intellectual history of major importance. Economists who find its topic remote from their immediate concerns still ought to read it. In contrast to a very distinguished

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Austrian economist, Wells powerfully argues that at least one vital human institution was the product *both* of human action and of human design.

Notes

1. All references to this book will be by page numbers in parentheses in the text.
2. See, for example, his *Individualism and Economic Order* (Chicago: University of Chicago Press, 1948). This theme is a near constant in Hayek's work.
3. F.A. Hayek, *Law, Legislation, and Liberty,* vol. I (Chicago: University of Chicago Press, 1973).
4. See F.A. Hayek, ed., *Collectivist Economic Planning* (London: George Routledge & Sons, 1935). Hayek contributed to this influential volume as well as editing it.
5. Walter Ullmann, *Principles of Government and Politics in the Middle Ages* (London: Methuen, 1961).
6. (Peru, Illinois: Open Court, 1987).
7. Ibid., p. 191ff.
8. Fichte's work is conveniently available in English, with an excellent preface by G.A. Kelly.
9. This work, first issued in 1943, appeared in a second "corrected" edition in 1952. In it, Heidegger left intact his reference to the "inner greatness" of National Socialism.
10. Hayek's "unintended consequences of human action" bear some affinity with Hegel's *Cunning of Reason* (*List der Vernunft*), though it would be wrong either to view Hayek as a Hegelian or to view Hegel as entirely within the Romantic camp. In complete contrast with the traditionalists and their latter-day followers are the members of the Erlangen School, who emphasize the conscious construction of mathematical and physical concepts. See Paul Lorenzen, *Constructive Philosophy* (Amherst: University of Massachusetts Press, 1987).
11. Hattiangadi, p. 30.

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The Politics of Hunger: A Review

Ralph Raico

States throughout history have persisted in severely encumbering and even prohibiting international trade. Seldom, however, can the consequences of such an effort—the obvious immediate results as well as the likely long-range ones—have been as devastating as in the case of the Allied (really, British) naval blockade of Germany in the First World War. This hunger blockade belongs to the category of forgotten state atrocities of the twentieth century. (Similarly, who now remembers the tens of thousands of Biafrans starved to death during the war of independence through the policy of the Nigerian generals supported by the British government?) Thus, C. Paul Vincent, a trained historian and currently library director at Keene State College in New Hampshire, deserves our gratitude for recalling it to memory in this scholarly and balanced study.

Vincent tellingly recreates the atmosphere of jubilation that surrounded the outbreak of the war that was truly the fateful watershed of the twentieth century. While Germans were overcome by an almost mystical sense of community (the economist Emil Lederer declared that now *Gesellschaft* [Society] had been transformed into *Gemeinschaft* [Community]), the British gave themselves over to their own patented form of cant. The socialist and positivist-utopian H.G. Wells, for instance, gushed: "I find myself enthusiastic for this war against Prussian militarism. . . . Every sword that is drawn against Germany is a sword drawn for peace." Wells later coined the mendacious slogan, "the war to end war." As the conflict continued, the state-socialist current that had been building for decades overflowed into massive government intrusions into every facet of civil society, especially the economy. The German *Kriegssozialismus* that became a model for the Bolsheviks on their assumption of power is well known, but, as Vincent points out: "the British achieved control over their economy unequaled by any of the other belligerent states."

Everywhere state seizure of social power was accompanied and fostered by propaganda drives unparalled in history to that time. In this respect, the

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Review of C. Paul Vincent, *The Politics of Hunger: The Allied Blockade of Germany, 1915–1919* (Athens, Ohio, and London: Ohio University Press, 1985).

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British were very much more successful than the Germans, and their masterly portrayal of the "Huns" as the diabolical enemies of civilization, perpetrators of every imaginable sort of "frightfulness,"[1] served to mask the single worst example of barbarism in the whole war, aside from the Armenian massacres. This was what Lord Devlin frankly calls "the starvation policy" directed against the civilians of the Central Powers (particularly Germany),[2] the plan that aimed, as Winston Churchill, First Lord of the Admiralty in 1914 and one of the framers of the scheme, admitted, to "starve the whole population—men, women, and children, old and young, wounded and sound—into submission."[3]

The British policy was in contravention of international law on two major points.[4] First, in regard to the character of the blockade, it violated the Declaration of Paris of 1856, which Britain itself had signed, and which, among other things, permitted "close" but not "distant" blockades. A belligerent was allowed to station ships near the three-mile limit to stop traffic with an enemy's ports; it was not allowed simply to declare areas of the high seas comprising the approaches to the enemy's coast to be off-limits. This is what Britain did on November 3, 1914, when it announced, allegedly in response to the discovery of a German ship unloading mines off the English coast, that henceforth the whole of the North Sea was a military area, which would be mined and into which neutral ships proceeded "at their own peril." Similar measures in regard to the English Channel insured that neutral ships would be forced to put into British ports for sailing instructions or to take on British pilots. During this time they could easily be searched, obviating the requirement of searching them on the high seas.

This introduces the second and even more complex question: that of contraband. Briefly, following the lead of the Hague Conference of 1907, the Declaration of London of 1909 considered food to be "conditional contraband," that is, subject to interception and capture only when intended for the use of the enemy's military forces. This was part of the painstaking effort, extending over generations, to strip war of its most savage aspects by establishing a sharp distinction between combatants and noncombatants. Among the corollaries of this was that food not intended for military use could legitimately be transported to a neutral port, even if it ultimately found its way to the enemy's territory. The House of Lords had refused its consent to the Declaration of London, which did not, consequently, come into full force. Still, as the U.S. government pointed out to the British at the start of the war, the Declaration's provisions were in keeping "with the generally recognized principles of international law." As an indication of this, the British admiralty had incorporated the Declaration into its manuals.

The British quickly began to tighten the noose around Germany by unilaterally expanding the list of contraband and by putting pressure on neutrals (particularly the Netherlands, since Rotterdam more than any other port was the focus of British concerns over the provisioning of the Germans) to acquiesce

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in its violations of the rules. In the case of the major neutral, the United States, no pressure was needed. With the exception of the beleaguered Secretary of State, William Jennings Bryan, who resigned in 1915, the American leaders were amazingly sympathetic to the British point of view. For example, after listening to complaints from the Austrian ambassador on the illegality of the British blockade, Colonel House, Wilson's intimate advisor on foreign affairs, noted in his diary: "He forgets to add that England is not exercising her power in an objectionable way, for it is controlled by a democracy."[5]

The Germans responded to the British attempt to starve them into submission by declaring the seas around the British Isles a "war zone." Now the British openly announced their intention of impounding any and all goods originating in or bound for Germany. Although the British measures were lent the air of reprisals for German actions, in reality the great plan was hatched and pursued independently of anything the enemy did or refrained from doing:

> The War Orders given by the Admiralty on 26 August [1914] were clear enough. All food consigned to Germany through neutral ports was to be captured and all food consigned to Rotterdam was to be presumed consigned to Germany. . . . The British were determined on the starvation policy, whether or not it was lawful.[6]

The effects of the blockade were soon being felt by the German civilians. In June 1915, bread began to be rationed. "By 1916," Vincent states, "the German population was surviving on a meager diet of dark bread, slices of sausage without fat, an individual ration of three pounds of potatoes per week, and turnips," and that year the potato crop failed. The author's choice of telling quotations from eye witnesses helps to bring home to the reader the reality of a famine such as had not been experienced in Europe outside of Russia since Ireland's travail in the 1840s. As one German put it: "Soon the women who stood in the pallid queues before shops spoke more about their children's hunger than about the death of their husbands." An American correspondent in Berlin wrote:

> Once I set out for the purpose of finding in these food-lines a face that did not show the ravages of hunger. . . . Four long lines were inspected with the closest scrutiny. But among the 300 applicants for food there was not one who had had enough to eat for weeks. In the case of the youngest women and children the skin was drawn hard to the bones and bloodless. Eyes had fallen deeper into the sockets. From the lips all color was gone, and the tufts of hair which fell over the parchmented faces seemed dull and famished—a sign that the nervous vigor of the body was departing with the physical strength.

Vincent places the German decision in early 1917 to resume and expand submarine warfare against merchant shipping—which provided the Wilson

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administration with its final pretext for entering the war—in the framework of collapsing German morale. The German U-boat campaign proved unsuccessful and, in fact, by bringing the United States into the conflict, aggravated the famine. "Wilson ensured that every loophole left open by the Allies for the potential reprovisioning of Germany was closed . . . even the importation of foodstuffs by neutrals was prevented until December 1917." Rations in Germany were reduced to about one thousand calories a day. By 1918, the mortality rate among civilians was 38 percent higher than in 1913; tuberculosis was rampant, and, among children, so were rickets and edema. Yet, when the Germans surrendered in November 1918, the armistice terms, drawn up by Clemenceau, Foch, and Pétain, included the continuation of the blockade until a final peace treaty was ratified. In December 1918, the National Health Office in Berlin calculated that 763,000 persons had died as a result of the blockade by that time; the number added to this in the first months of 1919 is unknown.[7] In some respects, the armistice saw the intensification of the suffering, since the German Baltic coast was now effectively blockaded and German fishing rights in the Baltic annulled.

One of the most notable points in Vincent's account is how the perspective of "zoological" warfare, later associated with the Nazis, began to emerge from the maelstrom of ethnic hatred engendered by the war. In September 1918, one English journalist, in an article titled "The Huns of 1940," wrote hopefully of the tens of thousands of Germans now in the wombs of famished mothers who "are destined for a life of physical inferiority."[8] The "famous founder of the Boy Scouts, Robert Baden-Powell, naively expressed his satisfaction that the German race is being ruined; though the birth rate, from the German point of view, may look satisfactory, the irreparable harm done is quite different and much more serious."

Against the genocidal wish-fantasies of such thinkers and the heartless vindictiveness of Entente politicians should be set the anguished reports from Germany by British journalists and, especially, army officers, as well as by the members of Herbert Hoover's American Relief Commission. Again and again they stressed, besides the barbarism of the continued blockade, the danger that famine might well drive the Germans to Bolshevism. Hoover was soon persuaded of the urgent need to end the blockade, but wrangling among the Allies, particularly French insistence that the German gold stock could not be used to pay for food, since it was earmarked for reparations, prevented action. In early March 1919, General Herbert Plumer, commander of the British Army of Occupation, informed Prime Minister Lloyd George that his men were begging to be sent home; they could no longer stand the sight of "hordes of skinny and bloated children pawing over the offal" from the British camps. Finally, the Americans and British overpowered French objections, and at the end of March, the first food shipments began arriving in Hamburg. But it was only in

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July, after the formal German signature to the Treaty of Versailles, that the Germans were permitted to import raw materials and export manufactured goods.

Besides the direct effects of the British blockade, there are the possible indirect and much more damaging effects to consider. A German child who was ten years old in 1918, and who survived, was twenty-two in 1930. Vincent raises the question of whether the miseries and suffering from hunger in the early, formative years help account to some degree for the enthusiasm of German youth for Nazism later on. Drawing on a 1971 article by Peter Loewenberg, he argues in the affirmative.[9] Loewenberg's work, however, is a specimen of psychohistory and his conclusions are explicitly founded on psychoanalytic doctrine. Although Vincent does not endorse them unreservedly, he leans toward explaining the later behavior of the generation of German children scarred by the war years in terms of an emotional or nervous impairment of rational thought. Thus, he refers to "the ominous amalgamation of twisted emotion and physical degradation, which was to presage considerable misery for Germany and the world" and which was produced in large part by the starvation policy. But is such an approach necessary? It seems perfectly plausible to seek for the mediating connections between exposure to starvation (and the other torments caused by the blockade) and later fanatical and brutal behavior in commonly intelligible (though, of course, not thereby justifiable) human attitudes generated by the early experiences. These attitudes would include hatred, deep-seated bitterness and resentment, and a disregard for the value of life of "others"—because the value of one's "own" life had been so ruthlessly disregarded. A starting point for such an analysis could be Theodore Abel's 1938 work, *Why Hitler Came to Power: An Answer Based on the Original Life Stories of Six Hundred of His Followers.* Loewenberg's conclusion after studying this work that "the most striking emotional affect expressed in the Abel autobiographies are the adult memories of intense hunger and privation from childhood."[10] An interpretation that would accord the hunger blockade its proper place in the setting for the rise of Nazi savagery has no particular need for a psychoanalytical or physiological underpinning.

Occasionally Vincent's views on issues marginal to his theme are distressingly stereotyped: he appears to accept an extreme Fischer-school interpretation of guilt for the origin of the war as adhering to the German government alone, and, concerning the fortunes of the Weimar Republic, he states: "That Germany lost this opportunity is one of the tragedies of the twentieth century. . . . Too often the old socialists seemed almost terrified of socialization." The cliché that, if only heavy industry had been socialized in 1919, then German democracy could have been saved, was never very convincing.[11] It is proving less so as research begins to suggest that it was precisely the Weimar system of massive state intervention in the labor markets and the advanced welfare-state institutions (the most "progressive" of their time) that so weakened the

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German economy that it collapsed in the face of the Great Depression.[12] This collapse, particularly the staggering unemployment that accompanied it, has long been considered by scholars to have been a major cause of the Nazi rise to power in 1930–33.

These are, however, negligible points in view of the service Vincent has performed both in reclaiming from oblivion past victims of a murderous state policy and in deepening our understanding of twentieth-century European history. There has recently occurred in the Federal Republic of Germany a "fight of historians" over whether the Nazi slaughter of the European Jews should be viewed as "unique" or placed within the context of other mass murders, specifically the Stalinist atrocities against the Ukrainian peasantry.[13] Vincent's work suggests the possibility that the framework of the discussion ought to be widened more than any of the participants has so far proposed.

Notes

1. Cf. H.C. Peterson, *Propaganda for War. The Campaign against American Neutrality, 1914–1917* (Norman, Okla.: University of Oklahoma Press, 1939), especially pp. 51–70, on propaganda regarding German "atrocities."

2. Patrick Devlin, *Too Proud to Fight: Woodrow Wilson's Neutrality* (New York: Oxford University Press, 1975), pp. 193–198.

3. Cited in Peterson, *Propaganda,* p. 83.

4. Cf. Devlin, *Too Proud to Fight,* pp. 158–67, 191–200; and Thomas A. Bailey and Paul B. Ryan, *The Lusitania Disaster: An Episode in Modern Warfare and Diplomacy* (New York: Free Press, 1975), pp. 27–33.

5. Cited in Walter Millis, *Road to War: America, 1914–1917* (Boston: Houghton Mifflin, 1935), p. 84. The U.S. government's bias in favor of the Allied cause is well documented. Thus, even such an "establishment" diplomatic historian as the late Thomas A. Bailey, in his *A Diplomatic History of the American People,* 9th ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1974), p. 572, states: "The obvious explanation of America's surprising docility [in the face of British violations of neutrals' rights] is that the Wilson administration was sympathetic with the Allies from the beginning." The partisanship of Wilson, his advisor Colonel House, Secretary of State Robert Lansing, and, especially, the American ambassador to England, Walter Hines Page, is highlighted in Bailey's even-handed account of the entry of the United States into the war (pp. 562–95). The reader may find it an interesting exercise to compare Bailey's treatment with that from a newer generation of "establishment" authority, Robert H. Ferrell, *American Diplomacy: A History,* 3rd ed. (New York: Norton, 1975), pp. 456–74. Ferrell gives no hint of the administration's bias toward Britain. Of the notorious British propaganda document luridly detailing the nonexistent German atrocities in Belgium, he writes: "It is true that in the light of postwar investigation the veracity of some of the deeds instanced in the Bryce Report has come into question" (p. 462). (On the Bryce Report, see Peterson, *Propaganda,* pp. 53–58, and Phillip Knightley, *The First Casualty* [New York: Harcourt Brace Jovanovich, 1975], pp. 83–84.)

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Ferrell's account could itself pass muster as somewhat refined Entente propaganda. Lest American college students miss the moral of his story, he ends with the assertion: "It was certainly in the interest of national security to go to war . . . logic demanded entrance."

6. Devlin, *Too Proud to Fight,* pp. 193, 195.

7. The British historian Arthur Bryant, writing in 1940, put the figure even higher, at 800,000 for the last two years of the blockade, "about fifty times more than were drowned by submarine attacks on British shipping." Cited in J.F.C. Fuller, *The Conduct of War, 1789–1961* (London: Eyre & Spottiswoode, 1961), p. 178.

8. F.W. Wile, "The Huns of 1940," *Weekly Dispatch,* September 8, 1918. Vincent notes that he is citing the article from a book published in Stuttgart in 1940.

9. Peter Loewenberg, "The Psychohistorical Origins of the Nazi Youth Cohorts," *American Historical Review* 76, no. 5 (December 1971): 1457–502. Loewenberg writes, for instance:

> The war and postwar experiences of the small children and youth of World War I explicitly conditioned the nature and success of National Socialism. The new adults who became politically effective after 1929 and who filled the ranks of the SA and the other paramilitary party organizations . . . were the children socialized in the First World War. (1458)

10. Ibid., 1499.

11. The leading advocate of socialization in Germany after the war was Emil Lederer, whose remarks about *Gemeinschaft* and *Gesellschaft* were cited previously. He denied, however, that the socialized economy would be more productive than capitalism. See Karl Pribram, *A History of Economic Reasoning* (Baltimore: Johns Hopkins University Press, 1983), p. 382.

12. The recent debate among German economic historians on this question is discussed in Jürgen von Kruedener, "Die Überforderung der Weimarer Republik als Sozialstaat," *Geschichte und Gesellschaft* 11, no. 3 (1985): 358–76.

13. *Historiker-"Streit." Die Dokumentation der Kontroverse um die Einzigartigkeit der nationalsozialistischen Judenvernichtung* (Munich: Piper, 1987).

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